VOL. VII, No. 3 SEPTEMBER, 1932

THE QUARTERLY REVIEW
of BIOLOGY

THE HEREDITARY FACTOR IN HUMAN NEOPLASMS

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I. INTRODUCTION

IT IS the purpose of this paper to present evidence that tumors, both benign and malignant, are inherited in man. Most of the evidence is concerned with neoplasms which are malignant, and for them the term "cancer" as used in the international lists of causes of death will be employed. It will not signify carcinoma, but will apply equally to malignant epithelial tumors and to sarcomata. Although numerous instances have been published in which more than one member of a family has been affected with cancer, they have not been widely accepted as evidence that cancer is inherited. Thus most text-books of pathology and of medicine consider either that there is no evidence, or that the evidence is too fragmentary, to justify any conclusion that there is an inherited basis for tumor growth. Most authors adopt the attitude that, inasmuch as one in every eight or ten persons who live past the age of 40, dies of cancer, it is to be expected on the basis of chance alone that occasionally several members of a family will exhibit cancer. They also point to the numerous cases of cancer in which there is no discoverable family history of the disease.

The fallacy which is present in these arguments will be exposed by evidence to be presented in some detail. It will be shown that, although on the basis of chance one might expect several members of a family to show tumors, one would not expect, on the basis of chance, all those affected in the family to show the *same type* of tumor in the *same organ*. There are many types of tumors and there are many organs throughout the body which these various types may affect, so that although the chances may be one in ten that any given person will be affected with tumor, the chances are only one in several hundred or even thousand that he or she will be affected with a specific type of tumor in a specific organ. Then when we go still further and consider the chances of two members of the same family having an identical type of tumor in the same organ, we find that the chances are still further reduced. In fact, in some of the rarer types of tumors, the chances become extremely remote.

For example, if a woman died in Canada

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in 1928, the chances of her dying from cancer of the mouth would have been only one in 2,000. They would have been only one in 1,572, of her dying of skin cancer. If a man had died in Canada in that year, his chances of dying of cancer of the breast would have been only one in 11,000, and if a person of either sex had died, the chances would have been only one in 7,800 that he or she would have died of cancer of the eye. Thus the old objection to the idea that cancer in man is inherited, which was put forward by many pathologists and physicians, is seen to vanish. The chances of two sisters or of a brother and sister dying of the same type of tumor in the same organ become so rare, that when such an occurrence takes place, it calls for an explanation. When it occurs frequently, as will be seen from the evidence presented in section III of this paper, the explanation that heredity, not chance, is the agent becomes imperative.

The fact that many persons show cancer whose parents were free from it is no proof that cancer is not inherited. Some of the reasons for the sporadic appearance of cancer will be considered later on, in section IV, but for the present we can offer an analogy between cancer and another disease which is inherited. Amaurotic family idiocy is almost universally recognized by the medical profession as being an inherited disease. It is one in which the parent *never* shows the condition, for the victim of amaurotic family idiocy succumbs in infancy; he or she never reaches maturity to become a parent. Yet there is no argument about this condition being inherited.

II. CHRONIC IRRITATION:—ITS RÔLE IN CANCER PRODUCTION

Before considering the evidence that cancer is inherited, the part which chronic irritation plays, or is supposed to play, in causing cancer, will be briefly reviewed. It will be shown that chronic irritation in some cases is undoubtedly a factor in the production of cancer. If, then, cancer is inherited, and if chronic injury may produce cancer, how are these two statements to be reconciled? What is the interrelation of these two factors concerned in tumor growth?

Undue emphasis has been placed upon the rôle of the environment as a causative factor in the production of cancer. Fallacious reasoning has been at the basis of many of these arguments. For example, lip cancer appears more frequently in men than in women. Men are, or at least were, smokers more frequently than were women. These two facts led to the erroneous conclusion that the chronic irritation of the lip by the hot pipe produced the cancer. The fact that only a small proportion of smokers ever developed lip cancer was ignored, also the fact that not all cases of lip cancer occurred in smokers.

Paraffin workers show a high percentage of skin cancer, especially of the thigh and scrotum; a percentage that is higher than the one found in men not engaged in the paraffin industry. Less than one per cent of paraffin workers develop this skin cancer, however, so that although exposure to the irritation of the chemical appears to induce growth in some workers, it does so in only a small proportion of them. Hence some factor other than the irritating oil on the skin must be present to cause cancer.

A woman with a history of breast injury develops breast cancer. She at once attributes the growth to the trauma. Many women with no history of injury die of breast cancer; and many women who have had serious injury to the breast die in old age with no signs of tumor growth.

Injury alone is not sufficient to cause cancer.

The site of tumor formation in men is found most frequently in the gastro-intestinal tract. If injury or chronic irritation were at the basis of cancerous growths, why should men show tumors of the digestive system, which is indifferent sexually, in greater numbers than do women? Not only is the percentage of intestinal tumors greater in the male, because the high percentage of breast and uterine cancer in the female naturally reduces the percentage of all other tumors occurring in women, but the actual number of men is usually in excess of the actual number of women affected with cancer of the stomach and intestines.

It is true that skin cancer can be produced experimentally in mice by the application of tar; and that tumors of some types can be transplanted from one animal to another. It is true that many workers with X-rays and radium have developed cancer. But not all animals so treated, not all X-ray workers have developed cancer. It may be that in these instances also, an inherited tendency to tissue proliferation must be present before the tumor growth appears. The fact that many persons develop cancer who have no history of injury, and that many persons with a history of injury never develop cancer impels us to hunt elsewhere than in the environment for the etiology of tumor formation.

The appearance of the same type of tumor in exactly the same organ, and at about the same age in identical twins, whose inheritance we know to be exactly the same, leads us to consider that there is an inherited tendency toward undue proliferation of certain tissues. Whether cancer can be induced to grow in an individual without this inherited background is at present rather difficult to say. That trauma need not produce it, even in those in whom we have the right at least to suspect the presence of the inherited background necessary for its appearance, is evident from a pedigree by Warthin. For two generations there had been cancer of the lip in the males of the family. In the third generation there were eight brothers. Seven of these were smokers. Thus in them was combined the history of trauma, and the possibility of the necessary hereditary background, although it was not certain that they had inherited the proper combination of factors for the appearance of cancer. Every one of them had passed the age of 65 and had no signs of lip cancer. The youngest brother was a non-smoker, but he developed cancer of the lip at 63. Thus in him the hereditary background was sufficient to produce the disease although there had been no trauma. Had the other brothers developed it, the tendency to emphasize the smoking as the causative factor would have been inevitable. They apparently had not inherited from their forebears the factors for the production of cancer, and so the irritation had been insufficient to elicit any growth.

In a second pedigree reported by Warthin, the rôle which trauma plays, provided the necessary background of inherited factors is present, is also brought out. Here again there had been cancer of the lip through two generations. In the third, there were four brothers, three of whom were smokers. These three developed lip cancer between the ages of 40 and 45. The fourth brother, who was a non-smoker, did not develop his cancer until the age of 63. Thus if the necessary inherited factors for the production of the cancer be present in the individual, the constant irritation to the susceptible part will induce the appearance of the growth at an earlier age than that at

which it otherwise would have occurred.

An excellent illustration of this point as well as of the one that cancer is an inherited disease is furnished by the record of Champlin. He reports sarcoma of the right testis in each of identical twins. One of the brothers had been struck in the right testis by a board. He developed sarcoma in this testis at 24 and died of it at 26. His twin brother had never had any injury to his testis, but at 31 he developed a sarcoma of the right testis, which was removed. He was alive and with no signs of recurrence 20 months later.

It has been found that in strains of mice in which mammary cancer is common, it appears at an earlier age in those which have suckled young than in those who have not. Thus environment does play a rôle, but it appears that its rôle is to bring to light a propensity that was already present, at a period earlier than it would have developed had the environmental factor been lacking. It is still a question as to whether the environmental agent is powerful enough to initiate growth without the presence of the inherited tendency.

The statements of Pack and LeFevre are not in accord, however, with this idea. They found in the analysis of many thousands of cases, that tumors of the reproductive organs occurred at a younger age in those who were not married than in those who were. Thus with cancer of the uterus, for example, the married woman developed it later, in their series, than did the unmarried woman. If repeated trauma, such as that of childbirth, were operating to induce an earlier appearance of the tumor, we should expect these age incidences to be reversed.

III. CANCER IS AN INHERITED DISEASE

There are so many facts which lend support to the idea that cancer in man is inherited that it is a matter for amazement that the heredity of tumors is not more widely recognized. (1) Although the fact that cancer in animals has been shown to have an hereditary basis cannot be used as proof that cancer in man is inherited, it may be used as additional evidence in the light of the arguments to be advanced. (2) Tumors tend to occur at approximately the *same age* in different members of the family. This is true even when the age at which the tumor appears in one family is far removed from the average age at which such tumors appear in the general population. (3) Tumors tend to affect the *same organ* in different members of the family. (4) The *same type* of tumor tends to appear in different members of the family; for example, if one sister has had fibroma of the uterus, her other sisters are more apt to show that type of tumor than they are to develop carcinoma of the uterus. (5) If one member of a pair of identical twins develops a tumor, the *other twin* is very apt to develop the same type of tumor in the same situation at about the same age. (6) There is an inherent capacity in some tumor cells, as retinal glioma, to develop early; in others, as carcinoma of the rectum, there is a capacity towards development late in life. We will now take up these points and submit the evidence from published clinical records. The first point, namely that tumors in animals have an hereditary basis, will receive no discussion here. Reference must be made to the numerous reports of such workers as Little and Slye for that information. Concerning the last point, namely that some tumors have a capacity for early development, while others de-

velop late in life, I will cite only two facts. The average age of death from all tumors is about 61. Retinal glioma practically never develops in a child that is past its seventh year. It frequently appears when the child is only a few months old.

Familial similarity in age of onset of cancer

There is a tendency for members in the same family, especially for members in the same generation in the family, to develop cancer at approximately the same age; provided that trauma has not induced growth in some members at an earlier age than that at which it would otherwise have appeared. Numerous examples might be cited. Warthin reported a family in which in one generation there were five brothers and sisters who died of cancer at the ages of 40, 42, 42, 45 and 55 years. In another branch of the same family two sisters died of cancer of the uterus at 40 and 44; in a third branch the three sisters succumbed to uterine cancer at 40, 40 and 42, while in still a fourth family, the two sisters died of carcinoma of the uterus at 35. The average age of death from uterine cancer is 55. This and all other averages computed through this paper are based on the vital statistics for the Dominion of Canada for 1928. It has been assumed that the population in Canada is one that is fairly representative.

Two brothers from this family tree died of intestinal cancer at 45, and two first cousins succumbed to the same disease at 37 and 28. The average age of death due to cancer of the intestines is 63. Thus these men not only died at about the same age, but they died from the disease more than 20 years sooner than the average age of death for males from the same cause. Moreover, the two brothers who died of intestinal cancer at 28 and 37 were brothers to the two sisters who died of uterine cancer at 35.

Instances of this sort might be multiplied indefinitely, showing that members of the same generation tend to show their growths at approximately the same ages. Thus not only is there inherited a factor for the production of cancer, and another for the organ in which it appears, but also what one might term a virulence factor as well; some forms of tumors as retinal glioma, appearing early in all patients; some, as cancer of the rectum or stomach, appearing early in some families, but late in others.

Thus in Warthin's family, 100 per cent of the women developing cancer of the uterus had shown it before 55. In the population as a whole, only 34 per cent of the women who are going to die of uterine cancer have shown it by the time they are 55. The same is true of the men who had gastric carcinoma in this family. One hundred per cent of the cases of gastric cancer which occurred in this family had developed before the age of 55; most of those who developed it having shown it many years before that age. In the population as a whole, only 21 per cent of the males who are going to develop cancer of the stomach, have done so at the age of 55. Thus we see that there is in this family a factor for very early growth of the tumor. It may be that the factor is one for excessive growth in the cancer cells; it may be that it is a lessened resistance factor in the normal tissues, but let it be what it may, there is no doubt that this family has an inherited tendency toward tumor formation, and furthermore it is a tendency which shows itself at an age which is much younger in this group than it is in the population as a whole.

Other examples in which the age incidence was about the same in the different

members of the family, thus demonstrating the fact that tumors are inherited and that the age at which they develop is also dependent upon an hereditary basis, follow. It will be noted in some cases as in those of Warthin just cited, that the age at which the tumor appeared in some families was by no means the average age at which such tumors appear in the population in general, thus making all the more striking the similarity in age incidence in the family. For example, tumors of the breast are rare before 30, but Tourneux reports a family in which the mother had had both breasts removed for fibroadenoma at the age of 14. Her two daughters had both breasts amputated for fibroadenoma, one being 16, the other 14 at the time of operation.

Contrasted with this is the report of a mother and her two daughters who had cancer of the breast at 76, 74 and 70 respectively. Thus there are two extremes presented, one in adolescence, the other in old age, but the one group occurs in one family, the other group in a second family. In the case of the identical twins reported by Burkard, the fibroadenoma appeared at the age of 21 in both sisters. Leavitt's twins were identical males, one dying at $6\frac{1}{2}$, the other at $8\frac{1}{2}$ years of cerebellar tumors. In Leschoziner's family, the mother died of breast cancer at 21, but not before she had passed on her heritage to three daughters; who when they grew up died at the ages of 14, 19 and 22 of breast cancer. Turning to tumors of the gastrointestinal tract, we again meet the extremes of age, but again we note that they are segregated according to family. Thus in Bickersteth's cases it was mother and son who had multiple polyposis of the rectum at the ages of 10 and 11 respectively, while in the families reported by Paulsen, the mother and father had died of gastric cancer at 59 and 60, while their six children developed cancer of the stomach at 54, 56, 60, 61, 62 and 63. In a second family reported by him, the mother had died of gastric cancer at 72, while five of the six children had died of gastric cancer at the ages of 60, 68, 68, 72 and 74. The sixth had died of cancer of the liver at 50. In Pfingst's cases, the tumor was a melano-sarcoma of the choroid occurring in two brothers at 44 and 47.

Thus from the standpoint of age incidence, we see that there is much to support the idea that cancer is inherited. Were it due to trauma alone, we cannot help wondering why trauma in one family should cause tumor of the breast in several members of the age of 20, while in another family it should cause cancer at 60. To say that one type of tumor develops in early life, while another is characteristic of later life does not answer the problem, for then we have to inquire why trauma should result not only in injury to the same organ, but in the development of the same type of tumor in that organ in several members of the family. We must recognize that the tendency to tumor formation is inherited, that tumors tend to occur in the same organ in different members of the family, that they tend to occur at the same age, and that they tend to fall into the same pathological classification.

Familial similarity in site of tumor

Rare tumors. Let us review briefly from the standpoint of their occurring in the same organ, and being the same type of growth, just a fragment of the evidence that tumors are inherited. Taking up some of the less usual forms of tumor, which therefore furnish the more striking examples of inheritance when they occur in more than one member of the family, we note first the family of

Sellei, in which a father and five of his seven children had sarcoma of the neck. Manson reported two brothers with lympho-sarcoma on the left side of the neck. Their mother had the same type of tumor in exactly the same situation. Ward reported a family in which two sisters and three brothers and the son of one of the latter had tumors on the acoustic nerve. Gardner and Frazier found a family, in which through five generations, comprising 217 persons, there were thirty-eight with tumors of the acoustic nerve. Arkin reported the case of a man, his son and his brother's son who died of mediastinal lymphogranuloma. Richards cited the case of a father and daughter who died of lymphosarcoma of the chest and abdomen. Block found cancer of the throat in a man and his two sons. The daughter in this instance had cancer of the uterus. Walker found sarcoma of the kidney in a brother and sister, while Berg reported tuberous sclerosis in which the tumors of the kidney were most evident, in a girl, her father and paternal grandfather. Tuberous sclerosis is very rare, and Spiller stated that in 1910 there had been only twenty-nine persons reported in the literature who were suffering from this condition. Twenty of these gave a history of other persons in the family similarly affected. Siemens reported tuberous sclerosis in a man, his daughter and her son, and in a father and daughter.

Brain tumors. Besides the tuberous sclerosis which affects the brain as well as the viscera, there is another very rare tumor of the brain known as angiomatosis. There have been only 60 cases of this reported in the literature, according to Møller, and many of them are grouped in families. Thus Møller himself reported a family in which a man, his three children and his two grandchildren, offspring of two different sons, were affected. Hipple had described a patient with cerebral angiomatosis. Schminke in 1931 added another chapter to this family history, when he recorded the death of the man's daughter and son from the same condition. Another report of this type of tumor is given in an editorial in the *Lancet* in which a man, his sister and his child all had angiomatosis of the retina and cerebellum. Hoffmann reported two brothers with brain tumor; and Reggiani reported a mother who died of a polymorphic-celled sarcoma of the right hemisphere of the cerebellum, and her son who died of a cystic glioma of the right cerebellar hemisphere.

Bone tumors. Tumors of the bone also show inheritance. Ransohoff reported the history of a woman who died of sarcoma of the femur. One son had an osteo-sarcoma of the neck of the femur, and his daughter had suffered from a spontaneous fracture of the femur which would never heal. There were also some instances of dissimilar tumor inheritance in this family. The woman of the first generation had a daughter who had cystic disease of the breast, and a second son with typical osteitis deformans. Werner reported a family in which a boy who had suffered from repeated fractures of the leg died of sarcoma which developed at the site of the injury. A second brother had had a similar history, and at 28 he broke his arm. A sarcoma developed at the site of the last injury. A sister of these boys had had numerous fractures in childhood, but had died at 10 of some infectious disease, before she arrived at the tumor age. The son of the second brother just referred to had also suffered from repeated fractures, which were followed by sarcoma and death at 20. In this instance, one who was inclined to lay stress upon trauma as the etiological agent, would call attention to the

numerous fractures which preceded the onset of the sarcoma. But it is only necessary to point out that there are on record hundreds of families, in which a tendency to repeated fractures is inherited, and in which tumor has never developed, although the number of fractures has run as high as 60. There must be a second factor present, namely the tendency to tumor formation, before the fractures became an efficient agent.

Eye tumors. Retinal glioma has already been mentioned, and in this there is striking evidence of inheritance. Thus Benedict reported this tumor in identical female twins. Zabugin reported three of twelve children affected; Boyd three of five; Thompson five of fourteen, the affected ones dying of the disease at 1½, 2, 3, 3 and 3½ years; Sym, a father and his three children, the latter all dying before the age of 4; Snell, five families, one with three affected, three with two affected, and one with mother and child as the victims. Purtscher (cited by Pel) recorded the case of a man who had had glioma of the retina, who finally died of sarcoma of the humerus. Of his eleven children, two sons and a daughter had glioma of the retina. One of the affected daughters had a son similarly afflicted. One of the normal sisters in this family had two of her nine children die of glioma. McGregor reported three of five children affected; Letchworth, a father and his three sons; the eyes having been removed before the age of 3 months in the last two children. Griffith cited the cases of several families in which this type of tumor had occurred. In one, a mother and four children were affected; in a second, a mother and three children; and in a third, a woman, her daughter and four of the latter's children were blind through glioma of the retina. One of the grandchildren had in addition a sarcoma of the jaw. Feinstein found a child and three of the mother's brothers and sisters with the condition, while Comas reported three sisters and two brothers who had had bilateral glioma, four of whom were dead; the fifth was still living but totally blind. There was a history of gastric cancer in the father, an instance of dissimilar inheritance as far as parent and children were concerned. Clausen collected many cases from the literature, which are here cited. Lerche reported four of seven affected; Sichel, four affected; Moher, three affected; de Gouvea, father and two of seven children affected; Steinhaus, brother and sister, and three paternal siblings; Hoffmann, mother and two sons affected; Owen, a brother and sister, their paternal uncle and his son; Newton, ten of sixteen affected; Calderini, three of fourteen affected; Leber, three of nine; Wilson, eight out of eight; von Graefe, two of seven; Marshall, three of six; Calderaro, four of five; Schönemann, Valenti and Fuchs, each two of three with retinal glioma. Records such as these should give food for thought to those who maintain that tumors are not inherited.

Retinal glioma is not the only thing to be noted as being among the hereditary tumors of the eye. Pfingst has reported melanosarcoma of the choroid in two brothers; Parker, sarcoma of the eyeball in two sisters; Davenport, choroidal sarcoma in a man, his twin daughters, and in the two daughters and two granddaughters of one of the twins. Thus through four generations there had been seven cases of sarcoma of the choroid. One sister of the twins in the second generation had had a tumor of the breast.

Skin tumors. Tumors of the skin are also inherited. Thus Sheppard found two brothers suffering from squamous-celled carcinoma of the mouth. There

were other tumors in this family, but they were in different organs. Thus the mother had died of breast cancer; a third brother had gastric cancer, and one of those who had carcinoma of the mouth had an adeno-carcinoma of the rectum with metastases to the liver. Wise recorded the presence of multiple endotheliomata of the skin located over the trunk and thighs in a father and his three sons. The two daughters were normal. Cystic benign basal celled epithelioma was found in a woman, her father, his two sisters and his mother by Gray. Heidingsfeld found adenoma of the sebaceous glands in two brothers, their two sisters, their father, and a brother and sister of their paternal grandmother. Other cases of adenoma sebaceum showing inheritance are given by Adamson, who found mother and son affected; by Balzer, who reported mother and daughter with the disease; by Brain, who found mother, son and daughter with the tumors; by Shelmire, who recorded father, son and three daughters with it; by Taylor, who saw three cases in one family; and by Mitchell, who reported it in mother and two daughters. Kerl recorded this disease in a family in which it could be traced back over four generations.

Tumors of the scalp were found through four generations by Ancell, who reported them in two brothers, their mother, her sister, her mother and her grandmother. Scalp tumors through three generations are recorded by Levit, who stated that the two parents in the first generation were both affected; their three children inherited the condition, and five of the ten children of the son in the second generation showed the tumors. Jenkins noted the presence of a skin papillus situated just in front of the external meatus of the left ear, ½ cm. in diameter, in a girl, her father's two brothers and one sister, her paternal grandfather's brother, and her great-grandfather. It had thus skipped two generations in the direct line of descent to appear again in her. Kerl reported a father and six of his eleven children with naevoid adenomata; Kroitschin, a woman and her two daughters with multiple nodular endothelial tumors; and Ledo, a family in which a woman, her two brothers and her son had cancer of the face, and her daughter's son and two daughters had epithelioma of the forehead and precancerous leukoplakia of the skin of the face, respectively.

Rodent ulcer of the face has been found in more than one member in the family by several authors. Adamson reported it in two brothers and a sister, their mother, grandmother and grand-aunt. Brooke cited the cases of two families, in one of which mother and daughter were affected, and in the second of which there were a mother and two daughters with the disease. Fordyce reported mother and daughter with rodent ulcer, and Gray cited the case of a man who had rodent ulcer of the face and one on the trunk. His maternal grandmother and his maternal uncle had both had cancerous nodules on the face, and his eldest brother had rodent ulcer of the face. Teratomata appearing on the surface of the skin were reported as present in a man and his sister's two boys. In the case of the uncle the tumor was situated on the sacrum, and in the two children it was attached at the root of the nose.

Liver tumors. Primary carcinoma of the liver, which is relatively rare, was reported by two different observers as having occurred in two brothers.

Uterine tumors. Tumors of the uterus are frequently found in more than one member of the family. Lawrie recorded the instance of five sisters who had come

to him for total hysterectomy because of fibroids of the uterus. Gregoire cited a family history in which through four generations the women had suffered from fibromata of the uterus. Auvray reported a family in which the woman in the first generation had died of cancer of the uterus at 89; her daughter had died of the same disease at 69; the daughter of this last woman in turn had succumbed to uterine cancer at 62. In the fourth generation the son of the woman dying at 62 had died of cancer of the rectum at 64. Williams reported a mother and her three daughters with cancer of the uterus. In a second family, there were three sisters dying of the disease at 32, 36 and 53 respectively. Their mother had died of it at 45; her sister and her mother had also died of uterine cancer. Sinkler recorded mother and daughter affected. Letulle found a family in which ten women had died of cancer of the uterus. He found fourteen other women who had cancer of the uterus, in whose family there was a history of cancer, but in some other organ.

Perhaps the most striking illustration of the heredity of uterine cancer is afforded by the pedigree of the *S* family, reported by Warthin. The males in this family succumbed to cancer of the stomach or intestine for the most part, and the females to ovarian or uterine cancer. Thus in the second generation, a woman whom we will designate as Mrs. A., and whose father had died of intestinal cancer, herself succumbed to cancer in the abdomen. Three of her daughters died of uterine cancer. Mrs. A's sister, Mrs. B., died of uterine cancer, and Mrs. B's two daughters died of the same thing. These two women (Mrs. A and Mrs. B) had four brothers. One was free from cancer himself, but one of his daughters died of cancer of the uterus. The other three brothers, who all had cancer either of the stomach or of the intestine, had between them four daughters who died of uterine cancer and one granddaughter who died of ovarian cancer. Since that report in 1925, Warthin stated that ten more members of the family had died of cancer, although he did not state just which ones had died nor did he state which organ was affected. But in four generations there had been thirty-eight deaths from cancer in this family. Not even the most ardent opponent of heredity would have an argument left in the face of a pedigree such as that.

It becomes all the more significant, when one views statistically, as Pearl did, the record of a family in which there had been eight deaths from cancer through a period of three generations comprising thirty-one persons. Twenty-five per cent would be a high mortality even at best, but the opponent of the heredity aspect of the question would say that on the average one of every seven adult persons dies of cancer, and that this death rate was less than twice that of the population in general. But Pearl points out the fallacy of such an argument. By comparing the death rate from cancer in the population at large, in which age groups similar to those succumbing in this family were considered, it was found that cancer was 196 times as frequent in this family as it was in the general population. By observing the age of death of the patients in this family of Warthin's, it is seen that on the whole they succumbed at an exceedingly early age, the men in the last generation dying at 27 and 30 from intestinal cancer.

Examining the statistics of Canada for 1928, we find that 562 men of the whole population of Canada died under the age of 55 of cancer of the stomach or intestines, and that 288 women died under 50 of uterine or ovarian cancer. Thus of deaths in Canada in 1928, 0.26 per cent were in

women under 50 with cancer of the uterus. At least 17 per cent of the deaths in this population of the *S* family were due to uterine cancer in women under 50. This figure represents the very minimum, for it does not take into account the ten deaths since that time from cancer. These were not included, since the sex, age of death and site of the tumor were not mentioned in Warthin's last report. The death rate was at least 65 times that of the general population for uterine cancer. Over 12 per cent, as the minimum figure, of the deaths in the *S* family had been due to intestinal or gastric cancer, a figure at least 24 times that of the general population rate. Thus we see how very high an incidence of cancer there was in this family,—evidence that there was a germ plasm, an inherited background, which predisposed to tumor formation greatly in excess of that found in the general population.

Gastric cancer. Cancer of the stomach is one of the most frequent of the tumors and so we naturally expect to find a great number of instances in which its inheritance has been recorded. It must not be assumed however that these cases of hereditary cancer in stomach or other organs are the only instances in which heredity of cancer has been found. There are hundreds of examples which occur, which never creep into the literature, and hundreds which are in the literature, which cannot be referred to here, because of lack of space. Paulsen reported gastric cancer in a man and his wife, and six of their seven children. The seventh was killed at 28 in an accident. In a second family, the father and six of his children had gastric cancer, and in the third family, the father, mother and three of their six offspring had died of cancer of the stomach. In the family reported by Wassink, there were sixty-nine cases of cancer, of which fifty-seven were in the stomach. Vignes recorded a family history, in which breast cancer, stomach cancer, and cancer of the generative organs were present. In the first generation a woman died of mammary gland cancer. She had two sons, who having no breast tissue to develop, did not reveal the tumor. One of them had inherited the factor, however, for his daughter died of breast cancer. The daughter in the second generation died of gastric cancer. This latter woman had had four daughters in turn. Two of them died of gastric cancer like their mother. The other two were normal, but nevertheless had inherited the factor in some degree, for the son of one of these normal daughters died of cancer of the testis, while the daughter of the other normal woman died of cancer of the ovary. Thus we find two cases of breast cancer, three of stomach cancer, and two of cancer of the gonads.

Pel reported several families in which there was evidence of inheritance of gastric carcinoma. In one, three brothers and two sisters had died of this disease, at ages varying between 43 and 64. In a second family, there were four brothers. Three of them died of gastric cancer and one of cancer of the nose. One of the three who had cancer of the stomach, married a woman who had no tumors herself, but whose two brothers had died of cancer of the face. They had nine children, three of whom had cancer of the stomach. One of the normal daughters in this family had a girl who died of gastric carcinoma.

Kaiser recorded a family with a similar history. Gastric cancer was the predominant type of tumor, although there were other organs involved. In the first generation, the mother and father were supposedly normal. They had nine children, of whom two were apparently free

from tumors. The eldest child was Mr. A., who died of cancer of the larynx. He had five daughters, two of whom had outspoken cancer of the stomach, and two of whom were suffering from symptoms that were highly suggestive of gastric carcinoma. The second child, Mr. B., had cancer of the sublingual gland. Mr. C. had cancer of the stomach, as had Mr. D. Mr. E. was normal, as was Mr. F., but Mr. F's son had symptoms that were due to cancer of the stomach in all probability. Mrs. G. had cancer of the stomach, and two of her four children were having trouble with their stomachs. Mrs. H. had cancer of the gall bladder, and of her seven children, two had trouble with the gall bladder, and two with the stomach. Mrs. I. had cancer of the stomach and one of her five children had developed cancer of the stomach.

Aebly reported two families, one with father and six children with cancer of the stomach, and one with both parents and five of the seven children with the disease. Reichert found a family in which both father and mother had died of gastric cancer, their son had died of the same disease, while their two daughters had cancer of the colon. Each of these daughters had a son, one with cancer of the colon, the other with cancer of the jejunum.

Again when we turn to the family reported by Warthin, the *S* family, we find numerous examples of stomach or intestinal cancer in one family. The originator of the family had died of either gastric or intestinal cancer. He had ten children; first, Mrs. A., who died of abdominal cancer, and whose two sons died of gastric or intestinal cancer, and three of whose daughters we referred to before as having succumbed to uterine cancer. The second person of the family was Mr. B., who had nine children. One of them was a daughter who died of stomach cancer, who in turn had a son with stomach cancer and a daughter with cancer of the ovary. Another of Mr. B's daughters had cancer of the liver and her son had cancer of the intestine. Still another daughter had cancer of the uterus and still another died of cancer, organ not stated. The sixth child in the *S* family was Mr. F., who died of intestinal cancer, and whose two daughters died of uterine cancer. Mr. G. died of gastric cancer, and his daughter died of uterine cancer. Mr. H. was normal but his daughter died of uterine cancer, and Mrs. I. had uterine cancer. Her two sons died of cancer of the intestine, her two daughters of cancer of the uterus, and one of the latter had two sons with intestinal cancer. It will be recalled that between the years 1925, when Warthin had brought the history up to the form in which it was just presented, and the year 1931, when he made his last report on this family, just before his own death, ten more persons had died of cancer.

Rectal cancer. Just as striking as the above records are those which deal with rectal carcinoma. Dukes has given a splendid article on the subject, in which he shows the intimate relation between multiple polyposis of the rectum and cancer. Almost half a century ago, Smith reported a group of two brothers and a sister with multiple polypi in the rectum, and the death of one of the brothers at 26 with adenocarcinoma. Despite such reports, as Dukes points out, the textbooks of Pathology and of Surgery for the most part ignore the hereditary aspect of the disease, and as was stated in the beginning of this article, the practitioners, or at least many of them, know nothing of the importance of heredity in cancer. Bickersteth recorded mul-

tiple polypi in a mother and son, some of them having been excised in the mother at the age of 10, and in the son at 11. There were many that were too high to be reached for excision. Oidtmann recorded three sisters with polyposis of the rectum; and in another family, mother, son and daughter with the disease. Hertz found three brothers and one sister with multiple polyposis; and Niemack, father and daughter. Döring reported a man and his sister who had cancer of the rectum. The woman's two children had polyps of the rectum, and the boy had them in the stomach, duodenum and colon as well. Childe recorded a father who had died of some rectal trouble, whose three daughters simultaneously developed carcinoma of the rectum. Watkins reported a family in which in the first generation the parents had died in old age, apparently free from tumors. They had four children. The first son died of obstruction of the bowel, the second died of some disease of the rectum, as did the third son. The daughter died of breast cancer. The second son had five children, one daughter dying of uterine cancer at 59, the second of breast cancer at 60, and two sons and a daughter dying of cancer of the rectum. The woman who died of breast cancer had a son who died of some type of abdominal tumor.

Lockhart-Mummery's cases, which Dukes has brought up to date, are as follows. In one family the father and mother had both died of rectal cancer. It was not known whether they had had polypi. They had ten children, four of whom died of rectal cancer. One of these sons with cancer had six children. Four were so far normal, one son had died of rectal cancer at 36, and one daughter had multiple polyposis without signs of cancer as yet. In another of his families, the father had died of rectal cancer at 42. He had nine children, seven of whom had rectal cancer, and five of whom had already succumbed to the disease. They had died at the ages of 46, 52, 39, 54 and 27. Their average age of death was thus 20 years below the average age of death from cancer of the rectum. One of them had a son with polyposis, but no cancer as yet, another had two daughters, both with multiple polyposis, one of whom had died of rectal cancer. In the third family, a man had cancer of the rectum. His paternal uncle had died of the same thing. He married a normal woman, whose father had died of cancer of the throat. They had two children, both with multiple polyposis.

Dukes cites a case of Jüngling which is remarkable, in that all the members of the family who were within reach were viewed with the sigmoidoscope, even though they presented no symptoms. It was amazing how many he found with polypi, who did not know they had them, but who were thus potential cancer cases. Herein lies a great field of preventive therapy, if only we can train our physicians to recognize the part that heredity plays in the production of disease, in cancer as well as in many others. In Jüngling's family, the parents in the first generation were not known to have had cancer. The father died of tuberculosis. There were six children in the family, two of whom died of tuberculosis, the other four of whom died of rectal cancer. The first of these was Mrs. A., who had polyposis as well as cancer. Her son had both conditions and died of the cancer. Mr. B. had rectal cancer from which he died, but it was not known whether he had polyps. His three children and three grandchildren were so far normal. Mrs. C., who had polyps and had died of rectal cancer, had seven children. Two daughters were normal.

A third daughter had died of either tuberculosis or cancer of the rectum. Since her son upon examination was shown to have polyps, the probabilities are that his mother died of rectal cancer superimposed upon a polypoid basis. The other three daughters and the son of Mrs. C. all had polyposis of the rectum, and two of the daughters each had one child with the same condition. Mr. D., the fourth member of the family, had polyposis and rectal cancer. Two of his three children were found to have polyps when examined with the sigmoidoscope. Thus in the third generation there were found six potential cancer patients, and in the fourth generation three.

One of Dukes' patients whose family was dying of rectal cancer, and who was found to have inherited the polyposis upon which cancer develops, had that portion of her bowel which was the seat of the growths removed as a preventive measure. Thus Dukes was able to get a person who was going to develop cancer, before the cancer appeared, and to remove the site of the possible growth. A knowledge of inheritance was at the basis of this preventive work. Had he not known that there was an hereditary background for these tumors, he would never have investigated those members of the family who were showing no symptoms.

Breast tumors. Tumors of the breast form no exception to the statement that cancer is inherited. Since breast tissue occurs only in the female, the inheritance of the condition may occasionally pass through a male who shows no cancer, but who has inherited the factor for it, which he may pass on to his daughters. This was found to be the case in the family reported by Vignes, in which the grandmother and granddaughter had breast cancer, the parent in the second generation being a normal male. Kaiser reported three sisters, their mother and grandmother with breast cancer; Wassink a great-grandmother, granddaughter and great-granddaughter with breast cancer; Leschoziner, a mother and three daughters who had died before the age of 22, with breast cancer; Tourneux, a mother and two daughters with fibro-adenoma which was removed at 14 in the mother and at 14 and 16 in the two girls; Smith, a woman, her two daughters and her brother's daughter with breast cancer; Dambrin, two sisters past the menopause, one married, the other unmarried, but both with bilateral cystic disease of the breast. A first cousin had been operated upon for the same condition. Finsterer found sarcoma of the breast in two sisters. Peiser reported a family in which the women had died of breast cancer through three generations. Moran found four sisters with carcinoma of the breast. Their father had died of cancer of the abdomen. Sinkler found two families in which mother and daughter had had intracanalicular adeno-fibroma of the mammary gland. Williams reported a woman and her five daughters with breast cancer. Wachtel reported a family in which a woman, her mother, her two aunts, her three first cousins, daughters of one of these aunts, her maternal grandmother and the latter's sister all had cancer of the left breast.

Horsley's three patients were sisters, and not only had the three of them cancer, but the cancer was in breast tissue that was misplaced in the axilla. Here then are two evidences of inheritance; one an inherited tendency to form accessory breast tissue in the axilla, and the second, an inherited tendency to tumor formation in that particular group of gland cells. Wassink found that in a

group of 258 women with breast cancer, seventy-six of them showed cancer in their nearest relatives. Of the seventy-six, thirty gave a history of breast cancer in their nearest relatives. On the basis of expectation from the incidence of breast cancer in the population as a whole, Wassink stated that only five of the thirty should have had a family history of breast cancer. Thus it occurred six times as frequently in these families as it did in the population at large.

Broca reported a family in which in the first generation, the woman died of cancer at 60. The particular organ was not stated. Her four daughters had cancer, two of them of the liver, the other two of the breast. The first had a daughter with cancer, site not stated. The second had three daughters with cancer of the breast, and one with cancer of the liver. Her son died of cancer of the stomach. The third daughter had three daughters and one granddaughter with breast cancer, one daughter with cancer of the uterus and a fifth with cancer of the liver.

Perhaps one of the most convincing records of cancer of the breast is that furnished by Power, for he recorded a cancer of the breast in the male. Cancer of the breast in women is sufficiently common, for the opponent of the idea that the tendency to form tumors is inherited to find a basis for his argument that occasionally we must find more than one woman in the family affected. But breast tissue in the male is so rare, that a history such as that of Power's leaves little room for debate. The father in this family died at 46 of cancer in the left breast. The mother died at 86 of "old age." The first son died at 65 of cancer of the throat; the second son of cancer of the left breast at 24; the first daughter at 63 of cancer of the right breast, the second daughter at 46 of cancer of both breasts; the third daughter at 40 of cancer of both breasts; the fourth daughter at 54 of cancer of both breasts; the fifth daughter was alive, but with cancer of the breast; as was the sixth daughter. The seventh daughter died in childhood, and the eighth died in early adult life in childbirth. The last child in the family was a son who had had a scirrhus carcinoma of the right breast removed two years before the time of the report, and who had just come back with a recurrence in the glands of the axilla. To find a father and two of his three sons with cancer of the breast is a strong argument in favor of its being an inherited condition. These numerous examples of inheritance of tumors have been given because there are many who argue that "One swallow does not make a summer," and that one family in which there were several cases of tumors does not prove that cancer is hereditary.

Tumors in identical twins

Investigating the proof for the argument that a tendency to tumor growth is an inherited factor, from the angle of tumor incidence in identical twins, we find a few records which deal with this question. Thus the identical twins reported by Benedict had retinal glioma. Those mentioned by Leavitt had cerebellar tumors. Burkard's twin sisters both developed fibro-adenoma of the same quadrant of the left breast at 21. Twinem found identical twins who had ovarian cyst at the same time on the same side. Joughin found identical twin girls with glioma of the cerebrum. Champlin found sarcoma of the right testis in both of male twins. Although there are but few of these cases mentioned here, they form very strong arguments in favor of the idea that heredity is the fundamental foundation upon which tumor growth rests.

Dissimilar tumors

It is true that in some families there are several cases of cancer, but different organs are involved. Numerous instances of this have been encountered as we were discussing the problem of the tumor appearing in the same organ in the majority of cases. A few other examples will be cited here. In one family reported by Paulsen, one sister had cancer of the breast, one had cancer of the intestine, the brother had cancer of the stomach, and his daughter had cancer of the breast. In another family the mother was married twice. She and her daughter by the first husband had gastric cancer. Her second husband had cancer of the eye, sarcoma of the shoulder and brain tumor. The two daughters by the second marriage had ovarian cancer. In a family cited by Roe, the father had cancer of the pylorus, the son cancer of the rectum, and the daughter, cancer of the breast. In Swoboda's family, the father and mother had died at 78 and 86 with no evidence of cancer. One daughter died of breast cancer at 59, a second of cancer of the intestine at 54, and the son of gastric cancer at 67. Letulle reported a family in which the woman in the first generation had cancer of the uterus; her son, cancer of the testis; her daughter, cancer of the ovary; another daughter, cancer of the anus and vulva. The son of the daughter who had ovarian cancer died of cancer of the prostate; and a daughter who was normal in the second generation had a daughter who had ovarian cancer. How are these cases of dissimilar inheritance to be explained, asks the person who does not believe that cancer is an inherited disease? How are we to explain the cases in which cancer appears with no family history of it? How are we to explain the skipping of generations in the appearance of the disease?

IV. THEORIES AS TO MODE OF CANCER INHERITANCE

In order to answer some of these questions, let us examine the theoretical aspects of the problem of heredity in cancer, and then consider some of the difficulties met with in studying its inheritance. We will also mention some of the objections of those who are either active opponents of the idea that cancer is inherited, or who feel that there is not sufficient evidence upon which to base a conclusion.

There have been many theories proposed to explain the inheritance of cancer: (1) that it is dependent upon a pair of unit recessive factors; (2) that it is dependent upon a unit dominant factor; (3) that it is dependent upon multiple factors, one combination being a dominant and a pair of recessive factors. The tendency toward tumor growth is supposed to be present in the recessive factors which are looked upon as being rather wide spread through the population. The site at which the tumor grows is regulated by the dominant factor, which is regarded as being much more restricted in its distribution.

Recessive inheritance

We will consider these theories and the objections to them one at a time. First, let us consider the theory put forward by Maud Slye, that cancer in mice, at least, is dependent upon a unit recessive factor. Were this correct, it would be necessary to postulate not only that this factor was responsible for tissue proliferation, but also for the site at which the overgrowth took place. It would not be sufficient for two persons bearing a factor for cancer to mate, in order to have cancerous offspring; they both would have to have a factor for cancer in the same organ. Such a theory would presuppose that when cancer developed in the breast of a woman,

both her parents must have carried a factor for cancer of the breast. In view of the numerous sites for cancer throughout the body, and of the numerous types of tumor which might appear at any of these sites, and finally of the number of the population who actually develop cancer of some particular organ, this theory would call for the presence of a great many factors for cancer of different organs in practically the entire population.

This theory might explain the appearance of cancer in a person, in whose family there was absolutely no history of cancer; but would it explain such a pedigree as that given by Warthin for the *S* family? In this the men had intestinal cancer, the women, uterine cancer. Were the disease dependent upon a pair of unit recessive factors in this family, it would mean that both parents of every man with intestinal cancer were carrying latent or obvious, a factor for intestinal cancer. It would mean that both parents of every woman with uterine cancer were carrying the factor for that condition. Not infrequently the brother in the family had intestinal cancer, the sister, uterine cancer. In this case, both parents would have had to carry factors for both types of tumors. In view of the great frequency with which these two types of cancer occurred in this family, and inasmuch as none of the mates selected by members of this family were themselves cancerous, the theory that cancer in this family was dependent upon unit recessive factors becomes untenable. The same objection applies to many pedigrees cited in this paper.

Dominant inheritance

Let us consider the evidence for the statement that cancer is due to a dominant factor. Were this universally true, no person should develop cancer, if neither of his parents showed it, provided that they lived to the age at which cancer could reasonably be expected to have developed. Now we know that many persons were victims of cancer, neither of whose parents showed it, although they lived to old age. Just at this point we encounter one of the difficulties which arise in the determination of the mode of inheritance of cancer. When can we say that a person has lived long enough to prove that he is not a cancer patient? We know that the average age of death from all causes in Canada for 1928 was 42.5 years. The average age of death from cancer was between 61 and 62 years, and had varied practically not at all from 1921 to 1928. Hence the average person has died almost 20 years before cancer has had a chance to become evident. Viewing it from another angle, we find that 62 per cent of the population have died before they reach the average age at which cancer claims its victims. Of those who live to be 62 or over, 11½ per cent will die of cancer. That means that had the remainder of the population lived to that age, the incidence of cancer among them would have been correspondingly high. If all the population lived to the age of 62, there would be a far greater number of instances of direct transmission of cancer from parent to child.

On the other hand, even when one has reached the age of 62 and has not developed cancer, it is very unwise to assume that one will not develop it at some later date. In Canada in 1928, there were 8,511 persons who died of cancer, and whose age at death was known. Of these, 1,270, or 15 per cent, did not die of cancer until they were between 65 and 69; 1,154 or 13.5 per cent, did not die of cancer until they were between 70 and 74; 909, or 10.6 per cent, died of it be-

tween 75 and 79; 426, or 5 per cent, did not show their cancer until they were between 80 and 84; 192 or 2.2 per cent, were between 85 and 89; 48 persons were between 90 and 94; 9 were between 95 and 99, and there was actually one who did not die of his cancer until he was past 100. Of the total number of cancer cases 47 per cent were past 65 before they died of the disease. Since the age of death from cancer does not in the vast majority of instances differ much from the known age of onset of the disease, we can readily see that many persons die before they reach the age at which they might have shown the disease, even when they live to very advanced years. There is always the possibility that had the person lived a few years longer, he would have succumbed. This insuperable difficulty arises whenever we attempt to analyze the mode of inheritance of this disease, for we can never be really certain that persons listed as free were actually free; they may have been potential cancer cases. Indeed the possibility must be considered that the whole human race is susceptible, having present within them factors for the development of cancer. They differ in that the age of onset in some comes within the range of the average human life, while in others it is set for so late a period that the average person has died before reaching it.

Thus the argument that many persons develop cancer whose parents did not have it, cannot be used as valid evidence against the idea that cancer is due to a dominant factor. In fact, in those families in which the age incidence is lower than it is in the general population, as it was in Warthin's *S* family, we do often find that the dominant mode of inheritance is much more nearly realized than in families in which the age of onset is late in life. On the other hand, we must recall that in retinal glioma, in which the age of onset is always in childhood, there are authoritative records of families, in which neither parent was affected, but in which several children died of the disease. In such families we can say without hesitancy that retinal glioma is not dependent upon a dominant factor.

Little considers that breast cancer in the human race exhibits the dominant mode of inheritance. Because of its being limited in its appearance to the female, its dominant mode of inheritance may be obscured. Thus it may be carried by a male, in whom it cannot be expressed because there is no breast tissue in which cancer may develop. He in turn may pass it on to his daughters, or to his sons, in which case it must then go through still another generation as a latent factor. Because of this difficulty, it becomes impossible to say that breast cancer in man is due to a dominant. It may be that a sporadic case of breast cancer has come down through a line of males to become evident in a female, or it may be that it is dependent upon recessive factors in that family, which are expressing themselves for the first time, due to appropriate matings.

An example of dominant inheritance in tumors is illustrated in the family reported by Gardner and Frazier. Here there were five generations with thirty-eight affected with neurofibroma of the acoustic nerve, an incidence that was at least 100,000 times as great in this family as in the general population. In this family, there was no instance of a child being affected unless it came from an affected parent. In view of these two facts, we are justified in concluding that neurofibroma of the acoustic nerve in this family was due to a dominant factor.

Multiple factors

There are those who consider that cancer is dependent upon multiple factors,

and that it is only when the proper combination of factors is present in the fertilized germ cell that cancer can develop. Such a theory would be in accord with the appearance of cancer in many of the pedigrees, where it occurs too frequently to interpret it as being dependent upon recessive factors alone, and where its occurrence is not always in accord with the assumption that it is due to a dominant factor alone. Warthin, in his last paper, considers this theory at length, and calculates how many of the population would have to be carrying the various factors in order that it appear with the frequency noted in the population at large.

Such a theory is attractive, although impossible of proof. It would explain, for instance, why the same organ is affected over and over again throughout a family, for this character is supposed to be due to the dominant part of the combination. It would explain why cancer is so frequently met with, for the recessive factor for tissue growth is supposed to be wide-spread throughout the population. It would explain the skipping of generations in some families, with the reappearance of the tumor at a later time. It would be obvious why in some families there was *dissimilar* tumor growth, some members being affected with breast cancer, some with cancer of the face. The parents were carrying two dominant factors for tissue growth, and one child inherited one, a second child inherited the other. This theory fits many of the facts that are difficult to explain on the basis of the first two considered.

Varying modes of inheritance in different families

Having discussed the current theories with respect to the mode of inheritance of cancer, and having pointed out how some pedigrees may be explained by one theory, which proves inadequate for the very next pedigree of cancer which we encounter, I should like to call attention to the fact that numerous other hereditary diseases met with in mankind exhibit, in one family, one type of inheritance, and in another family a totally different type of inheritance. Clinically and pathologically the diseases in the two families are the same. There is only one conclusion to be reached, namely, that exactly the same character may run through one family as a dominant, in another it may be dependent upon recessive factors, while in a third, it may be linked with the sex chromosome. There are many examples of this phenomenon, and some of them together with the correlated factors which substantiate its truth will be dealt with in a paper shortly to appear in *Human Biology*. If this be true for such clear cut diseases as amaurotic family idiocy or peroneal atrophy, there is no reason why it should not be true for such a protean disease as cancer.

Thus we may say that in one family retinal glioma is acting as a recessive character, in another family acoustic nerve tumor is acting as a dominant character, while in still a third family intestinal cancer may be difficult of interpretation on either basis. All the theories propounded for the explanation of the mode of inheritance may be correct for different tumors and in different families.

By the biologist, such an explanation will no doubt be rejected, for he is more or less accustomed to finding certain characters obey well defined laws; so much so that he even plots the arrangement of the genes in the chromosomes. In the human animal, there are many more chromosomes, and inheritance becomes much more complex. Many of the diseases which are inherited in man

can hardly be conceived of as being due to a single gene; they may be dependent upon hundreds of genes. If all these genes be located in one chromosome, then the disease will be transmitted as either a unit dominant or recessive. If, however, these genes, which we will say number from 1 to 100, are not all present in one individual, and if only 1 to 50 are there, then the disease will not be evident until the germ cell of that individual meets and fuses with a germ cell in which the complementary genes 51 to 100 are present. Thus in the first case, the disease might have acted as a dominant, being handed down from parent to child, but in the second instance, it would act as if dependent upon recessive factors, since both parents would have been free from the disease, and their children would have shown it. There is no reason why cancer, as well as many other inherited diseases and also normal characters, should not exhibit different modes of inheritance in different families.

Because of the difficulties involved in solving the problem of inheritance in human families with respect to any character as complex as cancer, there are those who feel that we must not draw any conclusions from human pedigrees. They claim that the number of persons involved is too small, that the number of generations which can be traced is too few, and that the evidence is too unreliable to make conclusions worth while. They argue that we cannot control the matings in order to test the validity of our hypotheses. It is true that we do not know the genetic constitution of any person with respect to cancer, for we do not know the factors which control it. If the factors are recessive, the cancerous person would be RR, if dominant, he would be either Dr or DD; and if a dominant and a pair of recessives are concerned then he would be either DDRR or DdRR. Much less can we estimate the genetic constitution of the person without cancer. Neither can we control the matings; but we can observe the effects of the matings made at random, and from them we can draw some conclusions that are not worthless.

One of the aids which the biologist is apt to overlook, is that furnished by statistical methods. It is true that if one attempts to determine the mode of inheritance by the use of statistics, he may encounter many pitfalls, but he may receive strong confirmation that the disease with which he is dealing is inherited. It is that point which I am most anxious to emphasize in this paper. As to the mode of inheritance, I consider that to be a subsidiary question at present, although calling attention to the fact that it is futile to attempt to interpret all pedigrees according to one theory.

V. STATISTICAL CONSIDERATIONS

For those who do not like the pedigree method, but visualize problems more clearly from the statistical angle, we might deal very briefly with some of the above data from that standpoint.

Take the case of the twins with sarcoma of the testis who developed their tumor by the time they were 30 or even before that. Looking at the statistics for Canada in 1921, we find that just about 60 per cent of the population was under the age of 30. Let us accept the figures for Canada as fairly typical of the population from which the cases recorded in this paper came. The total population in 1928 was 9,645,000. Hence about 5,700,000 were under 30. About half of these were males. We find that in 1928 there were in Canada nine males under 30 who had died of cancer of the entire genito-urinary tract. Certainly not

more than a third of these, or three, had sarcoma of the testis. Thus 1 in every 950,000 males under the age of 30 had sarcoma of the testis. Allowing two adult males to a family, which might not always be the case, that would mean that the chances were not more than 1 in 475,000 that this tumor should occur in brothers before the age of 30. Dividing this number by 10 would still give only one chance in every 47,500 that brothers would have sarcoma of the testis before they were 30. Thus the statement that two brothers died of sarcoma of the testis before they were 30 assumes an extreme significance when we view it statistically.

Again, consider the case of the mother and her two daughters who had had their breasts removed before they were 16. Using the population of Canada as a representative one for statistical purposes, we find that 9 per cent of the population is between the ages of 15 and 19, approximately 868,000 persons. A little more than half of these are females, or about 435,000. Not one of these in 1928, for example, had developed cancer of the breast. Thus the chances for a girl under 16 having cancer of the breast are less than 1 in 435,000, yet a mother and her two daughters were so affected.

Or take the mother and her three daughters who were dead of cancer of the breast before they were 22. Seventeen per cent of the population is between the ages of 14 and 22, the age limits in this group of four persons. Again approximately half are females, so that in Canada in 1928 we would have found about 822,000 women between 14 and 22. Of these only two had died of cancer of the breast. Thus the chances for women between these ages to die of breast cancer is about 1 in 411,000. Yet mother and three daughters had succumbed.

A man who was scornful of the idea that so many things were due to heredity remarked sneeringly, "I suppose you would say that you had seen a woman and her three daughters wearing skirts; therefore wearing skirts was inherited." Had we treated the matter statistically, we would have found 100 per cent of the women doing the same thing. That need not necessarily say that the wearing of skirts was or was not an inherited quality, but it would not argue so strongly in favor of its being inherited as the figures just quoted. To find four persons in one family dying of a disease before 22, which is not found except in one out of every 411,000 persons, is strong argument that there is in that family something unique which differentiates it from the rest of the population. That quality which is most peculiarly theirs, and distinguishes them most from all about them is their own inherent and thus inherited background.

So we might go on with many of the cases cited in this paper. The older the patient is when the tumor appears, the less striking becomes the evidence of inheritance, for the chances for cancer are so much higher in the population in general, that to find several in the family dying of cancer is not so convincing. But again when we analyse the cases from the standpoint of the *same* tumor occurring in the various members of the family, the familial incidence assumes importance.

Thus if we consider the mother and her two daughters who died of breast cancer at the ages of 70, 74 and 76, we find that statistical analysis emphasizes the importance of inheritance. There is about 1.6 per cent of the population between the ages of 70 and 76. Again using Canada's population as a representative cross section of the social order, we find that this would mean about 154,320 persons

or about 77,160 females between those ages. We find that 82 women died in 1928 from breast cancer between the ages of 70 and 76. Thus the chances for all women of this age were that 1 in every 940 would die of this condition. But in this family, three women died of it, probably three out of the only three women in the family. Hence, even in the advanced ages we see that analysis of the whole population as contrasted with the family under consideration shows that these families had a much higher incidence of cancer than the population at large.

Consider Power's family in which three males had had breast cancer under 46. There is 80 per cent of the population under that age, and that means that there were in Canada in 1928 3,858,000 males under 46. Two of these had cancer of the breast; so that one male in every 1,929,000 males under 46, is apt to get cancer of the breast. Yet a father and two sons had this disease, three males out of a population of four males, or 75 per cent. The fourth male had cancer, but it was of the throat, not breast. Thus this type of cancer was 1,500,000 times as prevalent in the males in this family as in the males of corresponding age in the population at large. That sounds very much more significant than when we say father and two sons had breast cancer.

Two brothers had sarcoma of the choroid at 44 and 47. "Mere chance," says the opponent of the idea that tumors are inherited. But analysis shows that the chances for a person between the ages of 44 and 47 having choroidal sarcoma are about 1 in 530,775. Thus that two brothers should have developed it becomes significant.

Pedigrees may be faulty; they may not extend over many generations; we may not be able to determine just what is the genetic basis of cancer; or what is the genetic constitution with respect to cancer of any person who has not the tumor or even of one who has it; but we must recognize that there is an inherited background for the development of cancer.

VI. EUGENICAL APPLICATIONS

Unfortunately there seems to be little hope from the eugenic standpoint of eradication, for usually the cancer does not develop in the patient until his children are mature, perhaps with all the family raised which they are going to have. On the other hand, one should not take too pessimistic a view of the fact that it is difficult to eradicate by selective mating. With the state of mind of the public as it is at present, there seems to be little hope that selective mating will breed out any disease. There are many diseases which make their appearance much earlier in life than does cancer, and which are recognized by the profession and laity alike as inherited; but that does not prevent the unaffected or the affected members either, from finding a mate. Moreover, we must face the fact that we have to die of something, and if we are saved from the terrors of cancer, we may be spared to lie as helpless invalids from cerebral hemorrhage. If we are snatched from an early death due to tuberculosis, we may live to develop a neoplasm of the brain. Something must inevitably take us off, and if the present tendencies continue, death due to an auto accident may materially lower the death rate from cancer as well as from many other diseases. If we can live a life of usefulness, of mental and physical well being until we reach three score or three score years and ten, then we have had our day, and if cancer comes to claim us, it may give us less of suffering, less of anxiety than many of the diseases which

are less dreaded. It is different with tumors that develop in early childhood, such as retinal glioma, which snatch a child from existence at an early age, or plunge him into a world of darkness for a life time. There eugenics has a definite rôle, and those who have had retinal glioma and survived the operation should not pass on the defect to their children; and parents who are normal, who have produced one child with retinal glioma, should not have any more to develop the same defect.

Proof has been submitted that cancer is due to an inherited factor. We will not discuss the question of whether it is cancer as such, or merely a predisposition toward cancer, that is inherited. From the practical standpoint they amount to the same thing, for we do not know the factors necessary to precipitate the cancer if the predisposition is present; or, what is more important, we do not know what steps can be taken to prevent its appearance, when the predisposition is present. Chronic irritation is by no means the only precipitating cause, for many cases of cancer develop in sites in which there has been no demonstrable irritation. We will take the stand, then, that there is passed on in the germ cell from parent to child a factor or factors which cause the cells in certain localities to undergo proliferation, to break loose from their original site and to set up a new abode in other tissues in the body. The age at which this occurs is also determined by a factor present in the germ cells, but it may be lowered by the introduction of extraneous factors.

VII. KNOWLEDGE OF INHERITANCE OF TUMORS OFFERS OPPORTUNITY FOR EARLY DIAGNOSIS

If it be true that cancer is inherited, and that there seems to be little hope of eradicating the disease by selective mating, is there any hope held out to the human race because we know that cancer is inherited? Or does such knowledge mean the doom of those in a family in which cancer has occurred? One pathologist states in his text-book that there is no proof that tumors are inherited, and so there is no cause for alarm. There is sufficient cause for alarm, even if cancer is not hereditary, when one views the excessively high mortality due to this disease. Rather than causing alarm, the knowledge that cancer is inherited should be looked upon as offering a ray of hope to those who are in cancer families of having the disease diagnosed early in their cases. One of the tragedies connected with cancer is that we do not know until too late, that we have the disease. We cannot study the early changes due to cancer, because we so seldom see it in its early stages. Now if we know that cancer is hereditary we have a selected population that is apt to develop the disease, if we take the relatives of those who have shown cancer. In them, by periodic examination, we may detect the cancer long before it otherwise would have been found. This offers a hope of early treatment. It is true that in most instances it does not hold out any promise of prevention, but that may come later. If we can train the physician and the public in the knowledge that cancer is inherited, we can get a very appreciable portion of those who will ultimately develop cancer, in the early stages of the disease, once we have trained them to come for periodic examination.

In one particular type of cancer, this knowledge offers prevention. Rectal carcinoma frequently develops upon the basis of polypous masses. Dukes has shown that these polyps exist for some

time before cancerous change sets in, and that one can prevent the appearance of the cancer by removing the section of the bowel in which the polyps are growing. Thus one is able to cure a cancer patient while he is still in the stage of being a potential, not an actual case.

Not only does the knowledge that cancer is inherited help in obtaining an early diagnosis, but it may ultimately pave the way for preventive methods. Just as we have found that there is an inherited state that is the precursor of pernicious anemia, we may find that there is recognizable a precancerous state which can be detected by clinical methods. This may be followed by therapy designed to prevent the cancer itself. This may be visionary, but our refusal to recognize that cancer is inherited, because it makes for pessimism, will not only not avert the disease, but it may destroy some of our best opportunities for early diagnosis and perhaps for ultimate prevention.

I desire to express my deep gratitude to my husband, Prof. Charles C. Macklin, for his constant suggestions and criticisms of this and numerous others of my papers on human inheritance. Not only has he inspired much of the work for which he has offered many helpful ideas, but he has frequently been of great assistance in the editorial revision of my manuscripts.

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THE PRESENT STATUS OF PROBLEMS RELATING TO THE CILIATES OF RUMINANTS AND EQUIDAE

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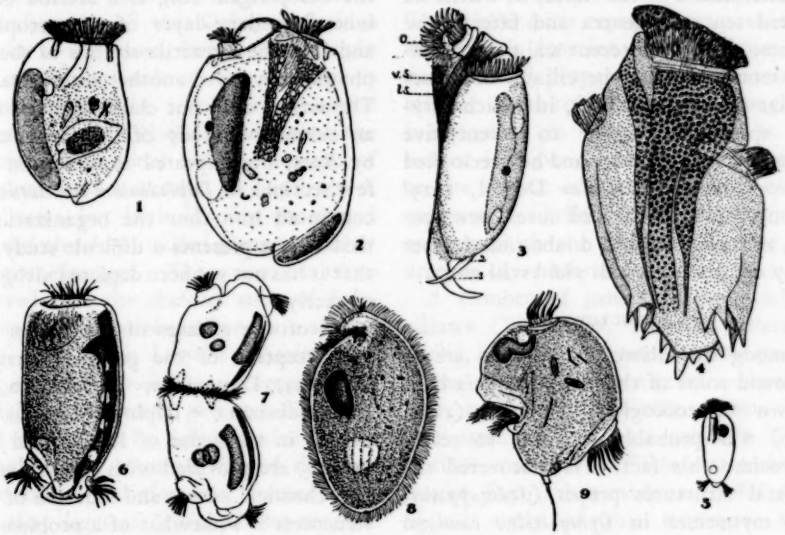
THE infusoria which inhabit the rumen and reticulum of ruminants and the caecum and colon of horses and the like constitute multitudinous and highly complex faunas. They were discovered by two Frenchmen, Gruby and Delafond, who made brief reports of their observations in 1843. The descriptions of the forms seen by these workers were inadequate, as judged by present standards, and it is difficult, though not impossible in all cases, to identify specific ciliates from the descriptions. It must be remembered, however, that knowledge of protozoan organization was not in a very advanced stage in 1843, for the classic works on unicellular animal life by Ehrenberg—with all its erroneous interpretations—and Dujardin had been published but a very few years previously.

Despite its shortcomings, the pioneering work by Gruby and Delafond had a tremendous influence upon certain phases of biological inquiry. In the first place, it marked the beginning of the study of protozoan life resident within domesticated animals. Secondly, it uncovered for taxonomic and cytological study an assemblage of unicellular organisms unexcelled in complexity and diversity by any other ecologic groups. Finally, it furnished the incentive for a considerable amount of speculation and experimentation on the possibility of the existence of a relationship of symbiosis or mutual aid between these entozoa and their hosts.

It is hardly necessary to comment at length upon the extent to which domesticated animals harbor parasitic protozoa. In addition to the ciliates under consideration, the rumen of an ox and the caecum of a horse may be inhabited by a number of species of flagellates and amoebae. An amoeba resembling *Entamoeba gingivalis* of the human mouth has been found in the mouth of horses. The intestinal mucosa of a cow may be invaded and severely damaged by one or more of five species of coccidia. A ciliate, *Buxtonella sulcata*, lives in the colon of calves. A microscopic examination of the blood of cattle and horses in certain parts of the world may reveal the presence of trypanosomes, and the so-called piroplasms which live within the red blood corpuscles. Again, the muscles of these animals in all parts of the world are usually infested with the sarcocysts of Sarcocystis. Gruby and Delafond probably saw Trichomonas in the dog and pig in addition to the ciliates of cattle, sheep, and horses, but one can be sure that they had no idea of the amazing extent to which animal microbes have adapted themselves to diverse locations in the animal body.

The first drawings of ruminant and horse ciliates were published by Colin in his great *Traité de Physiologie comparée des Animaux*. In the second edition of this work there appear eighteen drawings of cattle ciliates from which the genera Entodinium, Diplodinium, and Isotricha can be recognized, and fifteen drawings of horse ciliates from which the genera

Blepharocorys, Cycloposthium, and Paraisotricha can be recognized. The fundamental basis for the taxonomy of the ciliates of the rumen and reticulum was laid by Stein (1858–1867), who created the family Ophryoscolecidae with the genera Isotricha, Entodinium and Ophryoscolex, and described several species for each genus. The genera Buetschlia, Dasytricha and Diplodinium were added by Schuberg (1888), who also described a number of new species. Fiorentini (1889) added a few new species to existing genera. Crawley (1924) split the genus Diplodinium to erect the genus Epidinium. The most important taxonomic work on the family Ophryoscolecidae in recent years is Dogiel's (1927) monograph, in which sixty-five species and varieties from cattle are treated under four genera, with the genus Diplodinium subdivided into four subgenera. [Kofoid and Maclennan's (1932) further revision of "Diplodinium" has just come to hand.] When two species of Isotricha, three species of Buetschlia, and *Charon ventriculi* Jameson (1925) are added to the list, the number of valid species and varieties of ciliates which inhabit the rumen of the ox reaches the grand total of seventy-one. Of course, no one has ever found the entire number within one animal.



TEXT FIG. I. CILIATES FROM CATTLE (1–5) AND HORSES (6–9)

1, *Entodinium vorax*; 2, *Eudiplodinium affine*; 3, *Epidinium ecaudatum bicaudatum*, redrawn after Sharp; 4, *Ophryoscolex caudatus*; 5, *Charon ventriculi*; 6, *Cycloposthium bipalmatum*, after Hsiung; 7, *C. bipalmatum* in conjugation, after Hsiung; 8, *Paraisotricha colpoidea*, after Hsiung; 9, *Triadinium caudatum*, after Hsiung.

The infusorian population of the caecum of the horse yields nothing to that of the ruminant for diversity and complexity. Fiorentini (1890) was the first to name any of these protozoa. He erected the genera Didesmis, Paraisotricha, Spirodinium and Triadinium, to which he referred about ten new species. He also created four other new species, two for each of the preëxisting genera Entodinium

and Diplodinium. Bundle (1895), who was the best of the early systematic workers in the group, corrected Fiorentini's error in referring ciliates of the horse to the genera Entodinium and Diplodinium; and in addition he redescribed the genera Paraisotricha and Didesmis, and erected the genera Cycloposthium, Blepharocorys, Blepharocodon, Blepharoprosthium and Blepharosphaera. Gassovsky (1919) published an excellent though brief paper on the microfauna of the horse, in which he created ten new genera and fifteen new species. The most recent and comprehensive work covering the ciliates of horses is that of Hsiung (1930), in which fifty-one species belonging to twenty-five genera are recognized,—and he overlooked *Blepharoprosthium musculus* Dogiel, 1929! Hsiung himself described seven new species, and there is little doubt that further study of this fauna will yield still more.

STRUCTURE

Among the ciliates in question are to be found some of the most complex cells known to zoölogists. Günther (1899, 1900) was probably the first to really appreciate this fact. He discovered the skeletal structures proper (*Stützapparat*) and myonemes in *Ophryoscolex caudatus* and *Entodinium rostratum* of the sheep, and noted conjugation in *Cycloposthium bipalmatum* of the horse. Sharp's (1914) account of the structure of *Epidinium* (= *Diplodinium*) *ecaudatum* is familiar to many students of zoölogy. It will be remembered that he described skeletal laminae serving as true supporting structures for the entire body and for the retractile oesophagus, contractile strands for the retraction of the esophagus, and a truly remarkable neuromotor apparatus. The latter consisted principally of a deeply staining body, the motorium, just beneath the cuticle in the anterior region of the body, and fibrils leaving it and passing to the bases of the membranelles—groups of cilia which serve for locomotion and ingestion of food—and to a ring surrounding the esophagus.

Rees, (1931) working on *Diplodinium medium*, claims that the motorium described by Sharp is a fold of the middle layer of the ectoplasm, and that the body does not function as a coördinating center of a neuromotor apparatus; likewise, that the oesophageal ring is a section of the inner boundary layer of the ectoplasm, and that the retractile strands of the esophagus may have another interpretation. The writer does not claim to have made an exhaustive study of the issues raised by Rees, but repeated examination of a few sections of *Diplodinium ecaudatum* has convinced him that the organization of that ciliate presents a difficult study, and that it has not yet been depicted altogether correctly.

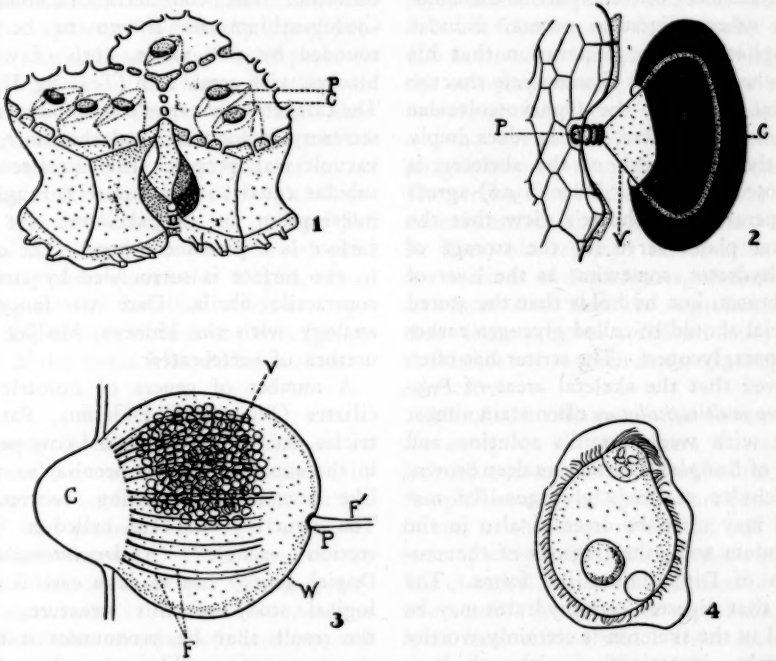
All of the ciliates of the rumen with the exception of the genera Buetschlia, Isotricha, Dasytricha, Entodinium, and Anoplodinium (= Diplodinium plus Eodinium, in the sense of Kofoid and Maclennan) are provided with skeletal plates. The chemical nature and function of these structures is somewhat of a problem, and is an issue around which there has been much controversy. Dogiel (1923, 1925—with Fedorowa—and 1927) considers that the skeleton consists of two parts—an outer cuticle impregnated with SiO_2 and a subcuticular portion made up of one or more layers of polygonal prisms arranged perpendicularly to the surface and composed of ophryoscolecin, a substance similar to cellulose except that it reacts more intensely to the ordinary chemical tests for cellulose. Sharp (1914) and Dogiel regard these structures as supports for the contractile strands of the esophageal wall and also for the entire body.

A radically different position is taken by Schulze (1924, 1927). He does not deny the skeletal function of the plates, but declares that in addition the honeycomb-like spaces in the skeleton serve for the storage of paraglycogen derived from the digestion of carbohydrates. He suggests that the structure may also secrete enzymes into the part of the endoplasm where digestion occurs. Schulze is emphatic in the declaration that his microchemical tests demonstrate that the skeletal parts of the Ophryoscolecidae contain no cellulose, but he does imply that the framework of the skeleton is composed of chitin. Trier (1926) agrees in general with Schulze's view that the skeletal plates serve for the storage of carbohydrates, somewhat as the liver of vertebrates, but he holds that the stored material should be called glycogen rather than paraglycogen. The writer has often observed that the skeletal areas of *Polyplastron multivesiculatum* often stain almost black with weak Lugol's solution and those of *Eudiplodinium magii* a deep brown. As Schulze realized, glycogen-like material may often be detected also in the operculum and other regions of the ectoplasm of Diplodinium-like forms. The view that digested carbohydrates may be stored in the skeleton is certainly worthy of further investigation, although it is doubtful if storage is its primary function.

The finer morphological study of the oligotrich ciliates of the horse is well under way. Strelkow (1929, 1931a and b) has contributed splendid detailed descriptions of the genera Cycloposthium and Tripalmaria. The well-developed plates of Cycloposthium are shown to be composed of a surface cuticle and, beneath it, a single layer of hour-glass-shaped structural units standing perpendicularly to the surface (Text fig. II, 1). The flattened outer and inner ends of these elements possess anastomosing processes. The outer surface of each element is perforated by tiny pores, which lead to minute cavities just within. The elements themselves are embedded in a structureless matrix. According to this author also, the skeletal elements are composed of ophryoscolecin; this in turn is said to be a form of hemicellulose. The contractile vacuoles of Cycloposthium are shown to be surrounded by two walls, both of which blacken with osmic acid (Text fig. II, 2). The thicker outer capsule is supposed to be secretory in function; and the inner, the vacuole wall proper. The latter sends a tubular canal to the surface through an interruption in the skeleton. At the surface is a permanent pore. The canal to the surface is surrounded by circular contractile fibrils. Dare we fancy an analogy with the kidneys, bladder and urethra of vertebrates?

A number of genera of holotrichous ciliates (Buetschlia, Didesmis, Paraisotricha, etc.) of the horse and cow possess in the anterior region a peculiar vacuole-like structure containing concretions. The structure has been called a "concretion vacuole" (*Konkrementenvakuole*). Dogiel (1929) has made a careful cytological study of this structure, with the result that he pronounces it to be an organ of equilibrium and not one of excretion as was formerly supposed. The entire structure is similar to a statocyst; the clump of granules, to a statolith (Text fig. II, 3). The vacuole itself is surrounded by a transparent cuticular cap at the outer pole and a thin membrane elsewhere except at a pore to be mentioned presently. The cavity of the vacuole contains a clear fluid in which is suspended a slightly yellowish clump of colorless, transparent concretions. The latter are readily soluble in one per cent hydrochloric and acetic acids, but are difficult

to dissolve in sulphuric or oxalic acids, suggesting that they may be composed of calcium carbonate. The vacuole wall is provided with fine fibrils. At one end of the vacuole is a pore through which a group of so-called centripetal fibrils passes from the vacuole wall to the interior of the endoplasm. Certainly Dogiel's description is reminiscent of the principle of the static organs of invertebrates.



TEXT FIG. II. 1, Skeletal units of Cycloposthium. *P*, pore to exterior from cavity, *C*; 2, a contractile vacuole of Cycloposthium. *P*, permanent pore in skeleton; *V*, wall of contractile vacuole; *C*, secreting capsule; 3, concretion vacuole of Blepharoprosthium-type. *V*, concretion mass; *W*, vacuole wall; *F*, fibrils of vacuole wall; *F'*, centripetal fibrils; *P*, vacuole pore; 4, "cyst" of *Paraisotricha colpoidea*. 1 and 2, after Strelkow, redrawn. 3 and 4, after Dogiel, redrawn.

CONJUGATION

The first account of conjugation among the ciliates in question and related kinds was given by Günther, who observed the phenomenon in six pairs of *Cycloposthium bipalmatum*. We are indebted principally to Dogiel and his coworkers, however, for most of our information concerning the nuclear and cytoplasmic behavior preparatory to, during and succeeding conjugation, as well as during binary fission. The processes have been worked out in whole or in part for *Opisthotrichum janus* of certain African antelopes (Cf. Dogiel, 1923), Entodinium, Diplodinium, Opisthotrichum and *Cycloposthium bipalmatum* of various hosts (Cf. Dogiel, 1925), *Dasytricha ruminantium* of cattle and sheep (Cf. Dogiel and Fedorowa, 1927), *Buetschlia parva* from *Camelus dromedarius* (Cf. Dogiel, 1928), *Paraisotricha colpoidea* and *P. magna* of the horse (Cf. Dogiel, 1930), and for *Didesmis quadrata*, *D.*

ovalis, *Blepharoprosthium pireum*, *B. musculus*, Isotricha, Blepharocorys, and *Polymorpha ampulla* (unpublished work by Dogiel). In most of the cases the conjugation is of the terminal type in which the conjugants join by the oral regions at an angle of 180°. In some, at least in Diplodinium and Paraisotricha (only one pair was observed), and in Dasytricha some time after the initial joining, the conjugants meet at a lesser angle. The process, in general, involves a progamous division in preparation of the preconjugants, pairing, formation of male and female pronuclei, synkaryon formation, separation of the exconjugants, and a metagamous development involving the forming of a new macro- and a micronucleus from the synkaryon of each exconjugant. Since the details in each case are too lengthy to be presented here, the interested reader is invited to consult the references in the appended bibliography.

PHYSIOLOGY

As in the case of most protozoa, very little is known concerning the cell physiology of the rumen infusoria. It is well known that most of them are extremely active at approximately blood temperature, and become immotile at room temperature. When in the immotile condition, the Ophryoscolecidae retract their membranelles. The pH of their normal environment averages between 7.6 and 7.8, according to Mowry and others. Ferber (1929) noted that, when the pH of the rumen content fell to 6.9 on account of depriving the animal of roughage, there was a simultaneous sharp drop in the numbers of infusoria. Mangold and Usuelli (1930) found that when milk or other acid producing substances were added to material from the rumen and kept in an incubator, no live protozoa could be found after the pH dropped to 6.0 or below. As will be mentioned later, this sensitivity of the infusoria to acid is the principle involved in the method of defaunating the rumen by putting the host on an exclusively milk diet. The souring of the milk provides the acid environment.

The intracellular digestive processes of the forms which ingest considerable quantities of plant fiber are of special interest, for they have direct bearing upon the rôle of the infusoria in the host. The early observers generally accredited the Ophryoscolecidae with the ability to digest cellulose in considerable quantities. Schulze (1924, 1927) and Trier (1926) also thought that they had observed the reduction of cellulose particles in the endoplasm of Ophryoscolecidae to a fine detritus which was later expelled from the anus. Dogiel (1925a), however, reported that even comparatively large cellulose elements of plant tissue, which had been ingested by the ciliates, were expelled through the anus intact and without any perceptible change in the sharpness of their outlines. He cautiously added the remark that he was unable to decide whether any chemical changes had occurred in the cellulose fragments which would make them more easily digested after they had passed to the outside of the protozoon. Usuelli (1930a) is skeptical concerning the ability of the rumen ciliates to digest real cellulose. The situation regarding starch digestion in the same organisms is quite clear. Trier followed the disintegration of starch grains inside the endoplasm and observed, with the aid of iodine staining, the subsequent storage of glycogen in the ectoplasm. Mowry (1930) reported that the addition of corn starch to the ration of goats receiving only hay resulted in a doubling of the numbers of infusoria.

The situation regarding the digestion of fat is not so clear. Trier observed that

droplets of milk fat were ingested by the infusoria along with considerable numbers of bacteria. The droplets in the endoplasm were digested at about the same rate and in the same manner as those free in the rumen. Consequently, he drew the conclusion that the fat was digested with the aid of the bacteria present in the endoplasm and not through an infusorian lipase.

Ferber showed that the addition of amides—as ammonium acetate and urea—to the diet of a ruminant did not effect an increase in the numbers of infusoria. Mowry reported a similar experience with asparagin. On the other hand, the addition of protein concentrates tends to increase the numbers of infusoria enormously, particularly when starch is also present. Both Ferber and Mowry observed this effect after supplementing a hay diet with grain. Mowry obtained greatly increased reproduction of the infusoria by adding Loeffler's dehydrated blood serum, wheat gluten flour, or casein to the ration of hay and corn starch, but dried egg albumin was ineffective. Many of the Ophryoscolecidae ingest and digest bacteria, and certain ones, as *Entodinium vorax* (Text fig. I, 1), are predacious upon other infusoria. Many forms also ingest plant particles and digest the protein and chlorophyll present therein. In fact, Ferber claims that chlorophyll is absolutely necessary for the metabolism of the rumen infusoria.

To summarize, it has been amply demonstrated that the rumen infusoria—particularly the Ophryoscolecidae—can digest starch and store it as glycogen. Trier's work indicates that they can also transform starch, glucose and lactose in solution into glycogen. The addition of many protein concentrates to the environment (in the rumen) provides an enormous stimulus to the multiplication of the infusoria, indicating that these substances are readily metabolized. The evidence indicates also that amides, certain amino acids, and certain proteins cannot be utilized. It is also doubtful whether these organisms can digest cellulose or fats.

TRANSMISSION OF INFECTION

The problem of cyst formation is intimately linked with that of the transmission of the infection from animal to animal. The early workers (Eberlein, 1895 and Günther, 1899) assumed that the source of the infection was resistant cysts which were present on the hay or water which the animals ingested. They noted that samples from the rumens of kids and lambs that had not learned to eat hay were free from the ciliates. The latter appeared in the rumen content after the habit of eating considerable roughage was developed. If the young animals were again put onto an exclusively milk diet, the ciliates vanished from the rumen, only to return when ingestion of roughage was resumed. Günther was unable to prevent reinfection, even when the hay was exposed to steam at 150°C. As will be brought out later, the methods of disinfection employed by Eberlein and Günther were probably ineffectual so far as removing the last source of infection from the rumen.

Neither worker was able to demonstrate the resistant forms or cysts which he believed to exist. Their idea of the source of these cysts, which they considered to be widespread in nature, appears to be somewhat vague. Apparently Eberlein considered the possibility that the ciliates of the rumen type also led a free existence, for he mentions that Certes claimed to have cultivated similar ciliates from dead leaves. We now know that neither the ruminant nor horse ciliates can long remain alive outside the animal body, for they are so extremely sensitive to cooling

that they will not live long at ordinary temperatures.

Liebetanz described and figured cyst-like bodies which he considered to be the cysts of the rumen infusoria, but it is now well known that he was dealing with the spores of fungi and worm eggs. A considerable number of investigators have failed to find the cysts in the rumen contents or the feces of cattle, sheep or goats or in the cecum, colon or feces of the horse. Among the recent investigators were Becker and Frye (1927, 1929), Winogradow, Winogradowa-Fedorowa and Wereninow (1930), and Hsiung (1930) who altogether examined material from a large number of cattle and horses. The writer once experienced the joys of the discovery of ciliate cysts in the feces of calves—only to learn soon afterwards that they belonged to *Buxtonella sulcata* Jameson (1926), a normal inhabitant of the cecum.

Dogiel (1929a) has come nearest to success in the search. In a particular sample of the cecal contents of a horse he found half the population of *Paraisotricha colpoidea* moving about within thin, rigid envelopes of irregular shape (Text fig. II, 4). He pronounces the phenomenon encystment,—perhaps either incipient encystment or for temporary protection against some transient unfavorable factor in the environment. He admits, however, that the structures cannot represent multiplication cysts or resistant stages.

The normal method of transmission of the rumen ciliates was solved, at least in part, by Becker and Hsiung (1929). Kids, which had previously been disinfected of ciliates by a starvation-copper sulphate method to be described presently, were fed unsterilized alfalfa hay, grain and water for weeks without becoming infected. The addition of unsterilized green grass, dried rumen content containing the corpses of the ciliates, and fresh and dried animal eliminations either to the diet or directly to the rumen contents by means of a tube failed to produce an infection. Defaunated lambs and goats in open pens and feeding upon ordinary alfalfa hay or turf did not become infected. Infection did not occur unless the kids were allowed to eat out of the same feed box and drink water from the same pail with an infected goat. Since the ciliates could be demonstrated microscopically in the saliva of lambs, the conclusion was reached that the infection is spread by contamination of the food with saliva containing the trophozoites, and that cysts of rumen infusoria either do not exist or are extremely rare. Dogiel and Winogradowa-Fedorowa (1930) and Winogradow, Winogradowa-Fedorowa and Wereninow (1930), however, reported that they could infect supposedly disinfected goats by feeding unsterilized hay, but not with hay which had been sufficiently sterilized. Mangold and Radeff (1930), however, confirmed and amplified the fundamental points in the results of the American workers, and their results support what they call the "contact infection hypothesis."

It is difficult to understand the inability of the Russian workers to maintain their animals infusoria-free on a diet containing unsterilized hay, unless their animals were not completely disinfected at the start. Mangold and Radeff (1930) and Mangold and Lenkeit (1931) noted the occasional occurrence of Colpidium-like ciliates in the rumen contents of disinfected lambs. It is hardly possible, however, that any worker of Dogiel's school would mistake free-living protozoa, living temporarily in the rumen, for the true rumen infusoria. Accidental infection of the experimental animals by contamination with the saliva of infected animals on the hands, clothing, and instruments of attendants or workers is also possible, a fact pointed out by Mangold and Radeff.

Likewise, save for the previously men-

tioned observations of Dogiel, there has been a general failure to discover the cysts of the horse ciliates. Hsiung has made the statement that trophozoites of a number of species commonly occur in an active condition in the fresh feces of the horse. Thus it would be possible for a horse to become infected by nibbling at the fresh feces of an infected horse—if only the trophozoites can make a safe passage through the acid environment in the stomach. It has been proved by Hegner that the trophozoites of certain other parasitic protozoa can pass safely through the stomach and intestine to the cecum of guinea pigs and rats. There exists, therefore, a strong possibility that trophozoites might arrive safely in the cecum of a horse by way of the oral route.

DISINFECTION OF HOSTS

Progress in solving the problems of transmission, host-specificity and physiological rôle in the host of the rumen infusoria would have been very much handicapped without the development of methods of disinfecting the experimental animals. Eberlein claimed that he could accomplish defaunation of the rumen by keeping the young animal on an exclusively milk diet for up to about seventy-two hours, but Dogiel and Winogradowa-Fedorowa have shown that at least seven or eight days is required for the complete removal of the fauna. The latter authors also discovered that a hunger period of six or seven days will disinfect the rumen of infusoria. Mangold employs methods similar to those of Dogiel. Günther (1899) and Liebetanz (1910) employed combinations of hunger and the introduction of acids (hydrochloric, citric and acetic) directly into the rumen as means of disinfection with a degree of success that is difficult to evaluate. The writer (1929) developed a method of defaunation which is to be recommended for its simplicity, effectiveness and safety. It consists in depriving the animal of food for four days, and on the third and fourth days introducing into its rumen through a tube down the esophagus a pint of water containing one gram of copper sulphate. Thereafter, the animal can be fed hay and grain without danger of the reappearance of the infusoria, providing contamination of food and drink with the saliva of infected animals is prevented.

Mangold and Radeff, in commenting upon the copper sulphate method, make speculations concerning the effect of possible oligodynamic action of the adsorbed copper in preventing reinfection, even in case infective material were introduced into the rumen. Their concern, however, is unwarranted and unsupported by any evidence. In fact, the writer has in his records an unpublished experiment in which a goat defaunated with copper sulphate ten days previously was accidentally reinfected with a fouled stomach tube. This tube had been used a few minutes before to obtain a sample of rumen contents from an adult goat which five days previously had completed an eight day period on an exclusively milk diet, the milk having been poured into the stomach through a rubber tube. Subsequently, infusoria reappeared in both animals. It should also be noted that Becker and Everett were able to reinfect lambs five days after they had been disinfected with copper sulphate in milk, and that Becker, Schulz and Emmerson (1930) had no difficulty in reinfecting their experimental goats within eight weeks or so after the defaunation process.

HOST SPECIFICITY

The parasitologist is constantly impressed with the limitation of parasites to particular hosts and to more or less definite

locations within the host. The genera and species represented in the rumen infusoria are limited to the paunch and reticulum, for the dryness of the content of the omasum and the acidity of the abomasum or true stomach are fatal to infusorian life in these regions. It is said that certain species similar to those in cattle may be found in hamsters in the oesophageal section of the stomach which is somewhat constricted from the true stomach. But do all species of ruminants have the same rumen fauna? The extensive studies of Dogiel (1927) answer decidedly in the negative; as one example of a possible many, Dogiel has never taken *Ostracodinium dentatum* from any animal except the cow. Dogiel lists sixty-five species and forms of Ophryoscolecidae from cattle, but only nineteen from goats and thirty-two from sheep. Is this unequal distribution due to specifically adapted infusoria, or to individual physiological properties of the various species of hosts?

Becker and Hsiung (1929) attempted to answer this question by inoculating the infusoria-free rumens of two defaunated goats with five species of infusoria from the rumen of a calf. All five species developed in the goat for two months, at the end of which one died out. Later a defaunated lamb was infected with the four species remaining in the goat. Dogiel and Winogradowa (1930) confirmed and extended these results. They successfully transferred to goats fourteen species of ciliates of bovine origin, five of which had never been taken from naturally infected goats. Four species from the cow failed to establish themselves in the goat. Thus it is to be concluded that the infusoria of domesticated ruminants show little, though some slight degree of, host specificity. Cross-infection experiments involving cattle, sheep and goats, on the one hand, and camels, deer and antelope of various species, on the other, would be extremely interesting.

Efforts by Becker and Hsiung to transfer eighteen species from the colon and feces of horses to defaunated goats were complete failures, as were also those of Dogiel and Winogradowa-Fedorowa to perform a similar experiment with infusoria from the cecum and colon of the horse. Neither does any mixing of the two faunas occur in nature. In fact, only one genus of ciliate, Charon, is common to the rumen of cattle and the large intestine of horses, but the species are different in the two cases. The occurrence of a species of Entodinium, *E. mamillatum* Cunha (1914), in the cecum of a South American rodent, *Cavia aperea*, represents an anomalous situation, to say nothing of the discovery of an Isotricha (*I. caulleryi*) in the posterior intestine of a cockroach by Weill (1929).

DO THE CILIATES BENEFIT THEIR HOSTS?

Gruby and Delafond were working on the physiology of digestion in domesticated animals when they discovered the ciliate faunas of domesticated ruminants and horses. Consequently, they ventured to suggest that the infusoria played a rôle in digestion; e.g., in the rumen and reticulum of the ruminant they subsisted upon the plant materials ingested by their host and converted them into the animal protoplasm of their unicellular bodies which, in turn, were sacrificed in large numbers to the digestive juices in the true stomach. Thus the protozoa furnished an animal element in the diet of cattle, sheep and goats. Similarly, the infusoria developing upon the plant material in the cecum and colon of horses were largely destroyed in the posterior part of the intestine.

Since the appearance of the paper by the two Frenchmen there have been many

speculations as to the rôle played by the infusoria in their host. Most of them are supported by only the slimmest evidence—often by mere observations of a suggestive nature—but they are of great interest, notwithstanding. Since these various views have been discussed at greater length in a paper by Becker, Schulz, and Emmerson (1930), it will suffice to recapitulate them here:

(1) Certain workers, notably Schwarz (1925), have developed the hypothesis that the infusoria in question convert amides and protein derivatives of plant origin, supposedly difficult to digest, into animal protein which is more easily digested.

(2) Other workers deny that the infusoria convert amides and protein derivatives over into proteins, but maintain on the other hand, that the microörganisms perform a service to the host by converting the proteins in plant food into the more easily digestible animal protein of their own bodies. This view is held by Mangold and his coworkers in Berlin.

(3) Zürn (1887) considered that the stomach infusoria were pathogenic to their host in that they created a catarrhal condition of the stomach and intestine. This view will not be discussed further, for it is now known that the presence of the infection is indicative of salubrious alimentation in the host, and its absence is usually noted only in cases of prolonged inappetence.

(4) List believed that the infusoria ingested large numbers of bacteria and molds whose inordinate multiplication would result in detriment to the host.

(5) A number of workers have declared that the infusoria assist in the process of digestion of cellulose which takes place in ruminants.

(6) Several physiologists aver that the only services which the infusoria can perform for the host consist of the mechanical agitation of the rumen content and the comminution of the particles of hay present therein. Dogiel (1925a) actually noted the disuniting of plant elements through the mechanical activities of the protozoa.

(7) A number of the earlier workers and, more recently, Becker, Schulz, and Emmerson (1930), Becker and Everett (1930), and Mowry and Becker (1930) hold that all the experimental evidence available supports no conclusion other than that the infusoria are harmless commensals, neither aiding nor injuring their host.

The conclusion just stated was supported by results obtained from three lines of attack upon the general problem. The first was in the nature of a comparative study of the digestibility of plant nutrients in goats for periods during which the rumen and reticulum held the normal fauna and during which the fauna was absent following artificial defaunation. Chemical analyses and careful weighing of the hay and grain furnished information as to amount of dry matter, crude fiber, alpha cellulose, hemicellulose, pentosans and protein fed to the goats. Analyses and weighing of the feces and urine, collected in "metabolism crates," determined the total amounts of the same materials eliminated by the animals. Thus it was possible to compare the coefficients of digestibility, or the percentage digested, of the protein and carbohydrate constituents of the food in normal and defaunated animals.

The average coefficients of digestibility in rounded numbers were as follows for four goats with the rumen infusoria: protein, 70; crude fiber, 38; hemicellulose, 48; pentosans, 50; alpha cellulose, 51. For the same animals during the absence of the infusoria the coefficients were:

protein, 68; crude fiber, 38; hemicellulose, 46; pentosans, 50; alpha cellulose, 49. It is evident that the differences between the normal and experimental series were too slight to furnish any support for a hypothesis to the effect that the infusoria are material aids to their host in the process of utilizing such food constituents as proteins, cellulose or hemicellulose. It is realized that there exist certain other factors which it was not possible for us to take into consideration, such as bacterial activity in the intestine. Just how much such factors would modify the interpretation of our results, if at all, cannot be estimated, for there is available no definite information as to the amount of decomposition of cellulose and proteins beyond the rumen.

The second line of attack consisted of a comparison of weight increases in two comparable lots of lambs—one with the normal ciliate infection and the other without it. If, as has been claimed by Ferber (1928, 1929) and his associates, the numbers of infusoria in the rumen are an aid to the host in their rôle of converting a certain amount of plant protein into animal protein, then a lot of lambs without the fauna should be handicapped in growth rate in comparison with a lot of normal lambs. This should be true, especially in view of Ferber's counts, which show that the numbers of infusoria in the rumen increase during periods of high protein utilization, as during rapid growth, pregnancy and lactation and then fall off during periods of low protein utilization, as during cessation of lactation (drying up) and old age. Our normal and infusoria-free lots consisted of seven lambs each. At the end of nineteen weeks the infusoria-free lambs had gained forty pounds more in total weight than their uninfected half-brothers! This weight advantage is, of course, not statistically significant of any ill effects due to the infusoria, for the number of lambs concerned was too small. The experiment indicates clearly, however, that young ruminants without the customary ciliates can utilize plant materials and actually increase in weight, probably without any handicap as compared with normals.

It is necessary to review further the already mentioned work of Ferber (1928, 1929 a and b) and his chief, Mangold, as a background for the third phase of the investigation. Ferber first showed that the infusoria of the rumen were unable to utilize non-protein, nitrogen-containing substances—as ammonium acetate and urea—in the synthesis of protein. On the other hand, he developed the hypothesis that the host derives benefit from the evident fact that the microörganisms are able to convert plant proteins into animal proteins. Certain physiological conditions of the host require increased protein metabolism. To help meet these requirements, an increase in the numbers of infusoria occurs. According to Ferber's counts, during the last six weeks of pregnancy and during lactation the number of infusoria in sheep and goats increased to about 2,000 per cmm. of rumen contents, about double the normal number, and when lactation ceased the number returned to about 1,000 per cmm., the "normal" number. A similar increase occurred during the growth of a lamb or kid until maturity was attained, and then the numbers fell off to about half the previous level.

Such was the content and experimental basis of Ferber's philosophy concerning the rôle of the ciliates. It is apparent that there are two weak spots in the hypothesis: first, it has never been shown, except for the lamb-growth experiments previously mentioned, whether or not ruminants can use plant and bacterial protein as well as infusorian protein; and secondly,

a plausible *modus operandi* for the correlation of the physiological state of the host and the numbers of infusoria has never even been suggested, a fact which Mangold (1929) himself admits.

Miss Mowry, working in our laboratory, has shown that the numbers of infusoria in the rumen respond to the nature and amount of the nutrients constituting the food of a goat. The details of her experiments are too extensive to be recounted here, but perhaps one or two pertinent examples will be amply illustrative. A particular goat maintained a level of about 118,000 ciliates per ccm. of a sample from the rumen, when the animal was on a diet of one kilogram of hay per day. After the goat had been supplied a daily ration of the same amount of hay plus 250 grams of a grain mixture for about a week, the number of infusoria rose to 1,771,000 ciliates per ccm. of a sample from the rumen. A daily ration of 250 g. of hay and 375 g. of grain produced an increase of ciliates to 2,972,000 per ccm. In another goat a daily ration of 500 g. of hay and 375 g. of grain swelled the numbers of infusoria to 3,156,000 per ccm. In this case they constituted 28.8 per cent of the volume of the sample!

Thus it was amply demonstrated that the numbers of infusoria are more directly correlated with the kind and amount of the food than with the physiological state. The facts suggest that the latter regulates the appetite; this in turn determines the kind and amount of food ingested; and to this, in turn, the infusoria respond. Mowry states:

> A critical study of the influence of diet upon the numbers of infusoria shows that the food plays a major rôle in conditioning the numbers of infusoria. . . . The infusoria live up to the margin of subsistence provided for them in the rumen. When the food is scanty or composed largely of plant fiber, the numbers are comparatively small. When there is an abundance of rich and easily digested food present in the rumen, the numbers of infusoria increase tremendously. The infusoria are thus "fair weather friends" of the ruminant, for they abound in numbers when there is an abundance of nutritious food.

Hence, we continue to regard the rumen infusoria as commensals, as also those of the cecum and colon of horses.

Reports of experiments by other workers have not been long in coming. Metabolism tests by the Russian workers Winogradow, Winogradowa-Fedorowa, and Wereninow (1930) on the digestibility of cellulose and protein in normal and defaunated sheep confirm in general the results of Becker, Schulz, and Emmerson, though one might wish that they were more comprehensive. Usuelli (1930), in Germany, determined by means of experiments *in vitro* that the amount of cellulose ingested by the rumen infusoria is of no quantitative significance. In fact, it is found to be doubtful if any digestion of cellulose occurs inside the endoplasm of these microörganisms. A conclusion similar to the latter had been reached by Dogiel in 1925. Thus the results of experimental work by foreign workers confirm those of the American group to the extent that they have reinvestigated the various phases involved in the problem. At present it seems clear that any claims for the existence of a true symbiosis between ruminants and the fauna in their paunch require further demonstration.

There is, however, one aspect of the association under discussion which deserves further consideration—if indeed this is not true of others. This is brought out by Usuelli's (1930) work in conjunction with hitherto unpublished observations by the writer. Usuelli (1930) fed sheep on ground barley. Six hours later he made a microscopic examination of the rumen contents to discover that over 84 per cent of the grains of barley starch had been ingested by the infusoria and

were in the process of being converted into glycogen. His earlier work with pulverized corn starch introduced directly into the rumen had given similar, though less striking, results. The writer has observed that lambs experimentally defaunated and on a diet of ground grain and alfalfa hay tend to show a greater rotundity of the body, as if somewhat bloated. The lambs were, apparently, in the best of health. The inference is that there may be a connection between the observations of Usuelli and those of the present writer, for the ingestion of the starch grains and the subsequent formation of glycogen inside the bodies of the infusoria may prevent the formation by bacterial fermentation of excessive amounts of CO_2 gas in the rumen. The point presents a possibility which may bear investigating in the future.

The successful cultivation of ciliates from cattle or horses has never been achieved—at least not until two years ago. The first to claim definite success is Margolin (1930). The writer and his students have met with discouraging results in their efforts to utilize Margolin's methods. This may be due to some factor vaguely called the "personal equation," so we do not wish to discredit his performance. It is disappointing, however, that he has not followed up his initial successes with further work, for a successful cultivation technique is the key to a host of interesting problems relating to the physiology and life-cycle of the organisms.

The significance of the faunas to their hosts presses itself forward as the most fundamental and fascinating problem of all. The complexity, specialization, diversity, and universal distribution of these protozoa—which occur inevitably and in large numbers in important sections of the digestive tract—naturally intrigue the zoölogist. The situation strongly suggests that these little creatures should signify either weal or woe for their hosts. Furthermore, it suggests an analogy of the ciliates concerned with the complex and admirably adapted gut flagellates which assist termites (white ants) to digest the cellulose of their woody diet. So the suspicion is bound to linger that the utility of the microfaunas of ruminants and Equidae will eventually be elucidated, although at the present time the preponderance of experimental evidence is to the contrary.

It is true that animal parasites often affect their hosts adversely in ways that are subtle, but none the less real. Similarly, it is possible that the ciliates favor their hosts in ways requiring a more exacting demonstration than the simple, striking proofs which Cleveland supplied for the indispensability of the flagellates to the termites. The counter possibility that the rumen and horse ciliates are "food robbers" to a limited degree would also be difficult to prove. At the present time, however, there is no more justification for believing that the microfaunas under discussion influence the economy of their metazoan hosts, than that Paramecium and other protozoa which dwell in the liquid of the pitchers of the pitcher plant affect that of the organism in which they dwell. There is this difference, however,—the former are committed solely to their respective habitats by virtue of extreme morphological and physiological specialization, while the latter are not.

The appended bibliography and those accompanying previous papers by Becker, Schulz and Emmerson (1930) and Hsiung (1930) together provide an almost complete record of all the literature concerning the infusoria of ruminants and the Equidae. A number of interesting facts regarding the cytology of these organisms

not mentioned in this discussion can be found in papers by Ten Kate (1928), Kraschenninnikow (1929), Campbell (1929, 1930), Strelkow (1931a), and Dogiel (1925b, 1927a). Information concerning the infusoria of animals closely related to the cow, sheep, goat, and horse will be found in papers by Dogiel (1927, 1928a), Strelkow (1931b), and Kofoid and Maclennan (1930, 1932). The last-named paper contains a revision of the genus Diplodinium.

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MODERN RESEARCH ON HUMAN TWINS

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THE occurrence of twins in man is a common phenomenon. In various countries the ratio of the birth of twins to the total number of births ranges from 1:80 to 1:90 (Blakeslee and Banker, 1930). Sometimes the twins are of the same sex, sometimes not. The percentage of unlike-sex twins varies between 26 and 36 per cent (Komai, 1928). Like-sex twins are therefore by far in the majority and this agrees with the assumption that there are two types of twins: fraternal and identical.

Fraternal twins are derived from two fertilized ova developing in the mother at the same time, and these may or may not be of the same sex. Identical twins are always of the same sex, because they are derived from a single fertilized egg-cell which later splits into two parts, each developing into a complete child. As a fertilized ovum has equal chances to be male or female and as there is no reason to assume that sex has anything to do with the formation of identical twins one may, with Weinberg, calculate the expected percentage of identical twins.

When 26 to 36 per cent of the twins are of different sex (and therefore fraternal) one may expect among the like-sex twins just as many fraternal twins. This leaves for the identical twins a percentage of 28 to 48 per cent.

THE CHORION AS A CRITERION OF IDENTITY

To distinguish between fraternal and identical twins in the case of those of the same sex, however, requires special consideration. Sometimes twins are born with a common chorion and for many years it was considered that these were identical twins. It happens that the percentage of monochorionic twins, however, is lower than the percentage of identical twins as calculated above. Various authors state the percentage of monochorionic twins as being 14 to 26 per cent (Komai, 1928). As the highest of these percentages is still lower than the lowest of the percentages calculated for identical twins there must be bichorionic identical twins also.

Many authors have been puzzled by this fact, although there seems to be a reasonable explanation. In Tatusia, a mammal which normally has identical quadruplets, the four young are monoamniotic, because the single germ from which they are derived splits at such a relatively late stage that a common amnion has already been formed. In man a similar process may occur. As an illustration I reproduce here a figure from Streeter (1919) in which very young monochorionic human twins are shown (fig. 1). Although in this case the division of the fertilized ovum probably occurred before the formation of the amnionic cavity, it may readily be understood that fission may occur even much earlier, for instance between the first blastomeres, before fixation in the wall of the uterus and before the formation of the chorion. In that case two partners of the twin will develop, each with its own chorion. It can hardly be expected that such a case will ever be observed in man, but in many animals of various types it is known from experimental embryology that each

of the first two blastomeres after separation may develop into a complete organism.

If this were all, all monochorionic twins might be accepted as being identical. A few of them, however, hardly can be so on account of their dissimilarity and so there is reason to assume that the chorions of two germs developing close together may fuse, in which case fraternal twins would also be monochorionic.

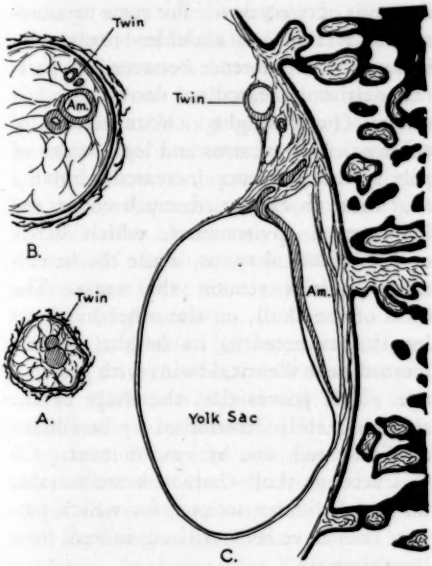

FIG. 1. SCHEMATIC DRAWING SHOWING THE PROBABLE STEPS IN THE FORMATION OF AN IDENTICAL TWIN

A. Stage showing a hypothetical twin budding off from the primary embryonic node. *B.* Older stage. *C.* A still older, actually observed case. The relatively small size of the twin in this specimen and the detachment of the yolk sac from the amniotic vesicle are indications of arrest in development. After Streeter (1919), Fig. 5.

SIMILARITY AS A CRITERION OF IDENTITY

If the chorion is no decisive means of distinction between fraternal and identical twins, the question rises whether there be other means. The degree of similarity may be one.

It is known that every organism is the product of its hereditary qualities and its environment. There is no reason why fraternal twins, even those of the same sex, should be more alike in hereditary qualities (genotype) than two brothers or sisters born from the same parents at different times. Only their environment is more similar and therefore, when fraternal twins resemble one another more than brothers and sisters in general, which they do only in a moderate degree, this may be considered as due to environment. Identical twins, on the other hand, have not only a similar environment but exactly the same hereditary endowment in addition, and they should therefore resemble one another much more than fraternal twins do.

I shall have an opportunity to show this in many respects. Whether one is dealing with fraternal or identical twins cannot be decided on the basis of one single character. In general, when a number of nonrelated characters are studied, the like-sex twins fall into two groups, one of which, composed of the identical twins, has a distinctly higher degree of similarity than the other group which contains the fraternal twins.

Here I wish to emphasize, with von Verschuer, the great importance of this fact. There is no hereditary quality which develops entirely independently from environment, nor is the environment able to create anything which is not potentially present in the genotype. Predestination by the genotype and realization by the environment, these are the factors which one meets everywhere in studying life. In botany or zoölogy, in anatomy or physiology, in phylogeny or embryology, in pathology or the study of normal life, there is no problem which does not involve the factors of heredity and environment. Therefore, it is of general impor-

tance to know how much is to be attributed to the one and how much to the other. In such organisms as can be cultivated, one may deal with material of the same genotype, in which all variation is due to environment, but in man one can merely reduce the variability of the environment. And the variability of the genotype, which is known to be very great, can only be avoided by studying identical twins.

As early as 1875 Galton mentioned the great theoretical importance of the study of twins, but in recent times von Verschuer, more than any one else perhaps, has emphasized this point, and his own investigations as well as his interpretations of the work of others are rapidly developing into a new chapter of human biology.

Before summarizing some of the results reached in the study of twins in various fields, it will be well to go more into detail about distinguishing identical and fraternal types of twins in the case of those of the same sex. This cannot be done very well at the time of birth, because important characters have not yet developed, and also because prenatal conditions are such as to make identical twins at birth more different than fraternal twins. The initial difference, for instance, between the two partners in birthweight and in length is usually greater in identical than in fraternal twins. This is probably due to intrauterine conditions, differences in placental circulation for example. Of two fetuses developing in the same chorionic sac, one is usually in a more favourable position than the other and consequently more advanced (compare the twins in fig. 1), and this accounts for the greater difference at birth between identical than between fraternal twins. After birth, however, the identical set of hereditary qualities tends to render identical twins ever more alike, whereas the differences in hereditary qualities between the partners in the case of fraternal twins tend to render them more different than before. In both cases the tendency of the environment is to make the partners more different with growing age when the number of environmental differences is constantly increasing.

Von Verschuer (1928a) twice investigated 42 pairs of identical twins with intervals of two years. For some measurements, stature and shoulder-breadth, for instance, the difference between the partners neither increased nor decreased. For others (body-weight, circumference of thorax, length of arms and legs, width of pelvis) the difference increased, showing that these characters are much under the influence of environment, which varies even in identical twins, while the hereditary qualities remain the same. The form of the skull, on the other hand, its length, its breadth, its height, became more alike in identical twins with growing age. This proves that the shape of the skull is mainly determined by hereditary qualities and not by environment. On this account skull characters are suitable for racial differentiations, for which purpose they have been utilized, indeed, for a long time.

According to von Verschuer (1928b) the best age to test twin resemblance is from 4 to 6 years. Not one or two but a large number of independent characters should be studied. For the white race experience has pointed to the following characters as being especially suitable: color, shape and implantation of the hair, color of the eyes, color and vascularization of the skin, shape and position of the teeth, shape of the nose, the lips, the hands, and finally the blood-group.

It is well known that fingerprints are used for the identification of persons. They are suitable for this on account of their widely ranging variability and for

that very reason they are unsuitable for distinguishing identical from fraternal twins. At most the prints of nine fingers have been found to correspond, even in identical twins. Komai (1928) has shown, however, that a very special case of variation in the fingerprints of twins, or even better in their palm- and sole-patterns, may justify the conclusion that one is dealing with identical twins. That is when right and left side of each twin show marked differences but with close resemblance between both right and both left sides. As each organism has only one genotype the environment is responsible for the differences between right and left, but the correspondence between the right and left sides of the two partners shows that their reaction upon differences in environment is the same, and this points to identity of genotype.

Once the decision between identical and fraternal twins has been made from a study of the characters mentioned above the resemblance may be studied for any other character. Some interesting results have been collected in the following tables for which the summary on twin-research by von Verschuer (1931) has offered the material. In these tables, *I* means identical twins, *F* fraternal twins, concordant means that both partners did or did not show the character concerned, discordant that one partner showed it and the other one not. Slight difference means that both partners are nearly concordant. All twins referred to in the tables are like-sex twins, identical as well as fraternal.

LIKENESS IN PHYSICAL CHARACTERS

Table 1 gives information regarding six normal characters. In every case the majority of the identical twins are concordant, whereas discordancy occurs much more frequently in fraternal twins. This shows that the characters concerned are more under the influence of heredity than of environment. Generally speaking, the partners are concordant when their genotype is the same, as in identical twins, but when their genotype may be different, as in fraternal twins, the partners are more often discordant.

The epicanthus of the eye presents a special case. From race crossing between the Chinese and the white race it is known that the epicanthus behaves as a single recessive Mendelian character. In other words, the appearance of an epicanthus is not controlled by the environment, but by the presence or absence of a single gene. Under such circumstances identical twins can never be discordant, any more than they can be of different sex. Indeed, among 14 pairs of identical twins none was found to be discordant, whereas among 10 pairs of fraternal twins 6 were in that position. In cases of Mendelian inheritance discordancy is sufficient to prove that a certain pair of twins is fraternal, but concordancy on the other hand means nothing, as fraternal twins may both happen to have the same hereditary endowment in the case of the particular character in question.

Twin-research provides a certain control in genetical investigations and it also may suggest indications for further research. When, as shown by Table 1, among 109 identical twins none are discordant for freckles, we have an indication that the inheritance of freckles is a simple case of Mendelian inheritance.

In regard to Table 1 it may be remarked that the shape of the heart has been studied by means of x-rays.

LIKENESS IN PATHOLOGICAL CHARACTERS

Table 2 refers to seven characters which may be called pathological, all of which are very much under the influence of the genotype, more so than they are under the control of environment. In order to show

TABLE 1

CHARACTER	NUMBER OF CASES	CONCORDANT	SLIGHT DIFFERENCE	DISCORDANT
Dimple in chin	29 I	27		2
	32 F	15		17
Dimples in cheek	23 I	18		5
	23 F	15		8
Freckles	109 I	93	16	0
	107 F	51	27	29
Tuberculum impar of 1st upper molar	70 I	65	4	1
	98 F	30	16	52
Shape of heart	55 I	32	17	6
	45 F	5	25	15
Epicanthus of the eye	14 I	14		0
	10 F	4		6

TABLE 2

CHARACTER	NUMBER OF CASES	CONCORDANT	SLIGHT DIFFERENCES	DISCORDANT	PERCENTAGE CONCORDANT AND MEAN ERROR	DIFFERENCE IN PERCENTAGE CONCORDANT
Hernia	39 I	24		15	61.53 ± 7.78	61.53 ± 7.78
	6 F	0		6	0.00 ± 0.00	
Hypertrophy of tonsilla	64 I	39	11	14	78.12 ± 5.16	48.12 ± 7.85
	60 F	11	7	42	30.00 ± 5.91	
Rachitis	22 I	20		2	90.90 ± 6.13	67.83 ±10.28
	26 F	6		20	23.07 ± 8.26	
Measles	189 I	179		10	95.72 ± 1.48	8.74 ± 3.15
	146 F	127		19	86.98 ± 2.78	
Whooping cough	100 I	96		4	96.00 ± 1.95	13.11 ± 4.74
	76 F	63		13	82.89 ± 4.31	
Feeblemindedness	16 I	14		2	87.50 ± 8.26	80.84 ±10.47
	15 F	1		14	6.66 ± 6.43	
Sentenced for criminality	13 I	10		3	76.92 ±11.68	65.16 ±14.05
	17 F	2		15	11.76 ± 7.81	

this in a simple way I have calculated for each case, for the identical as well as for the fraternal twins, the percentage of concordant cases and its mean error, and also the differences between the percentage of concordant cases among the identical and the fraternal twins and their mean errors. When these differences surpass three times

their mean errors they may be said to be of statistical significance, indicating that identical twins are more concordant than fraternal twins and that the character concerned is greatly influenced by the genotype.

Thus in the case of hernia, hypertrophy of the tonsils, rachitis and feeblemindedness, whatever the environment may contribute to the realization of these pathological conditions the importance of the hereditary basis is undeniable.

Even for a "character" like being sentenced for criminality, where we are on the borderline between the social and biological sciences, the preponderance of the biological hereditary factors is shown.

For the two infectious diseases, measles and whooping cough, the result is less clear. Strictly speaking, in this case there is no significant difference between identical and fraternal twins, because the difference is less than three times its mean error, and the figures for other infectious diseases are even less convincing. But in these two diseases at least there is an approach to a significant difference.

DISCORDANT CHARACTERS

The characters of Tables 1 and 2 have been chosen for demonstrating the influence of heredity. Others, however, do not show such influence at all. As seen in Table 3 there is no difference between identical and fraternal twins in the proportion of concordant and discordant cases for naevi spili (coffee-brown spots of the skin) and for the whorl of the headhair. A clock-wise whorl occurs much more frequently than a counterclock-wise one. Therefore in both identical and fraternal twins the chances for concordancy (both partners with clock-wise whorl) are greater than for discordancy, but there is no significant difference between identical and fraternal twins and therefore the environment must be largely responsible for the turn of the vortex of the headhair. Cases like this show that identical twins may be just as remarkable for their lack of similarity as for their close resemblance for which they are popularly renowned.

Table 2 showed that feeblemindedness is greatly influenced by heredity. This is true for mental diseases in general. Table 4 proves this for schizophrenia and mania. But there is one mental disease, mongoloid idiocy, which does not show any heredi-

TABLE 3

CHARACTER	NUMBER OF CASES	CONCORDANT	DISCORDANT
Naevi spili	56 I	21	35
	52 F	21	31
Headhair whorl	163 I	112	51
	123 F	101	22

TABLE 4

CHARACTER	NUMBER OF CASES	CONCORDANT	DISCORDANT
Schizophrenia	34 I	31	3
	8 F	3	5
Mania	25 I	24	1
	2 F	1	1
Mongoloid idiocy	1 I	0	1
	22 F	0	22

tary factor. The 23 pairs of twins of Table 4 are all discordant for this condition, which makes environment the responsible factor in the case.

But it is not necessary to study only characters which may be present or absent, and which therefore offer an alternative. Developmental processes may also be compared with the same result of demonstrating the influence of environment and heredity. Table 5 gives the results of Korkhaus, who studied the eruption of

teeth in twins. The difference in time of the eruption of the first tooth as well as of the eruption of the permanent teeth is less between partners of identical twins than it is in fraternal twins.

LIKENESS IN INTELLIGENCE

The influence of heredity on the intelligence is also demonstrated by the work of von Verschuer (1930a) and others who measured the intelligence by some well-known tests. By multiplying the intelligence age by 100 and dividing this figure by the chronological age one obtains a figure called the intelligence quotient (I.Q.). Table 6 shows that the difference between partners in the intelligence quotient is less in identical than in fraternal twins. As intelligence is not the manifestation of a single character, but rather a reflection of the whole personality, the cooperation of heredity and environment here is very intricate. Von Verschuer reaches some interesting conclusions, one of which may be quoted here. The intelligence tests involve memory. Twin-research proves that persons of the same genotype (identical twins) retain very similar memories of all the various impressions which they receive. People with different genotype, however, choose different portions of a similar experience for storage in their memory. In connection with such problems it is an unusual but very sound idea of Blakeslee and Banker (1930) to establish experimental schools for identical twins, taught if possible by identical twins. In large cities, notwithstanding the heavy differential mortality against twins, there should be a sufficient number of identical twins of grade school and even of high school age for such a purpose.

TABLE 5

Difference in time of eruption of the first tooth

	0–2 DAYS	1–2 WEEKS	1–2 MONTHS	MORE THAN 2 MONTHS
35 I	27	6	2	0
24 F	9	7	7	1

Difference in time of eruption of permanent teeth

	NONE	SOME DAYS	2 WEEKS TO 1 YEAR	MORE THAN 1 YEAR
23 I	8	9	6	0
21 F	0	4	15	2

SUSCEPTIBILITY TO TUBERCULOSIS

Important results from twin-research have been obtained also by Diehl and von Verschuer (1930) in their study of tuberculosis. They investigated 19 pairs of identical twins and 56 of fraternal twins, all 75 belonging to families with tuberculosis. Their ages ranged from 0 to 57 years. In the age period below 11 years there were 23 pairs of twins (6 I and 17 F), in that from 11 to 20 years were 25 pairs (7 I and 18 F), between 21 to 30 years were 15 pairs (3 I and 12 F) while between 31 to 60 years were 12 pairs (3 I and 9 F). These twins are arranged in three groups. For details the original paper should be consulted, but here it may be stated that in group I there was complete agreement in

TABLE 6

Difference in intelligence quotient between twins

	0–1	2–3	4–5	6–7	8–9	10–11	12–13	14–15	16–17	18–19	20+
52 I	17	17	7	7	2	0	1	0	0	0	1
48 F	6	12	4	10	6	3	1	3	2	0	1

pathological condition between partners, in group II there were differences, and in group III one partner was ill or had died from tuberculosis, whereas the other one was healthy. When no attention is paid to age (Table 7) the majority of the identical twins is in group I, whereas groups II and III are more numerous in fraternal twins. This shows that hereditary factors influence the disease, a fact which is demonstrated even more clearly when the groups of different ages are compared. The number of twins in group III, where the differences are greatest, is higher for identical twins below 20 years of age than for those above 20 years. For the fraternal twins the reverse is true. Here one observes the same phenomenon as shown by certain bodily measurements: identical twins converge with growing age, fraternal twins diverge. This is due to their genotype, as the environment tends to become more and more different for both identical and fraternal twins.

TABLE 7

Tuberculosis in 75 pairs of twins

ALL AGES			LESS THAN 20 YEARS OLD			MORE THAN 20 YEARS OLD		
Group	I	F	Group	I	F	Group	I	F
	per cent	*per cent*		*per cent*	*per cent*		*per cent*	*per cent*
III	10	42	III	15	26	III	0	71
II	21	37	II	15	43	II	33	24
I	69	21	I	69	31	I	67	5

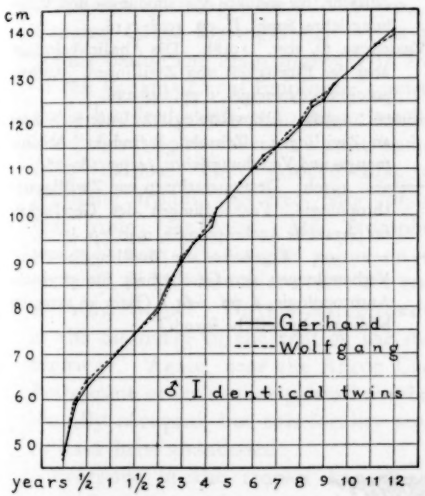

FIG. 2. DEVELOPMENT OF STATURE FROM BIRTH TO AN AGE OF TWELVE YEARS IN MALE IDENTICAL TWINS
After von Verschuer (1930 b) Fig. 8

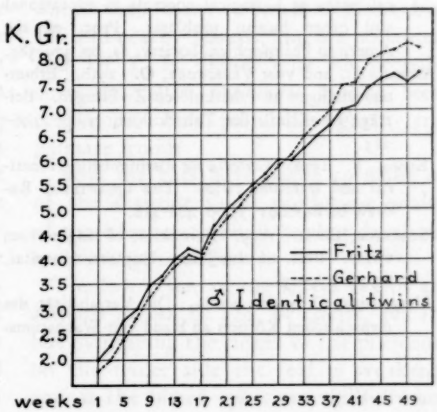

FIG. 3. DEVELOPMENT OF BODYWEIGHT DURING THE FIRST YEAR IN MALE IDENTICAL TWINS
After von Verschuer (1930 b), Fig. 7

Von Verschuer's study (1931) of measurements and proportions of the body in

twins has led to the general conclusion that again identical twins resemble one another more than do fraternal twins. That some characters are much more influenced by environment than others is illustrated by figures 2 and 3. Figure 2 shows the growth curves for stature in the case of a pair of male identical twins; the two curves are nearly coincident. Figure 3 gives the growth curves for bodyweight for a pair of male identical twins, and the resemblance is striking, although the difference is greater than in figure 2. This latter difference is not to be wondered at when one recalls how much the bodyweight is under the influence of environment.

Space prevents going into details, but one further conclusion may be pointed out, namely that a difference in stature of 7 cm. between two men is due to hereditary differences in 997 out of 1000 cases. In only three cases in a thousand is the environment responsible for this.

It is to be hoped that twin-research in the future will be continued on a much larger scale than it has in the past. Our knowledge of human heredity would greatly benefit from it and experimental medicine might find in it valuable support.

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A PECULIAR ROOSTING HABIT OF BATS

By T. BARBOUR

Museum of Comparative Zoology, Harvard University

WHILE I was in the Canal Zone this year my friends Dr. Herbert C. Clark, Director of the Gorgas Memorial Laboratory in Panama City, and Mr. J. H. K. Humphrey, Assistant Chief Quartermaster of the Canal, asked me to go with them to see some palm trees under the drooping leaves of which bats were roosting. Indeed Humphrey had kindly had one of these trees photographed (fig. 1) by Mr. E. Hallen, the Panama Canal Photographer. This tree stands beside the main highway in front of the Panama Canal Employees' quarters but a hundred yards or so before reaching the Pedro Miguel railroad station coming from Balboa. They noticed that the drooping fronds sheltered bats for the first time last year. Mr. Humphrey noticed them first in the palms on the premises of Mr. Bouchier across the track at Pedro Miguel. Further search resulted in the discovery of the tree on the main highway. Again, near the Ancon Canal Restaurant other similar palms have been recently occupied, but this locality seems to have been abandoned.

Everyone in the tropics knows that some bats live only in the deepest and darkest caves while other species, more tolerant of light, spend the day in trees with heavy dense foliage such as nispero or sapote trees, or under palm leaves. To this group of species belongs *Uroderma bilobatum*, the typical subspecies of which is common in the Canal Zone. Goldman (Smith. Misc. Coll. 69, 5, 1920, p. 199) writes, "In the forest near Gatun *Uroderma bilobatum* was located, a few in a place, clinging during the day in clusters to the midribs on the underside of large palm leaves. They usually choose darkened spots where *the leaf is folded over* (ital. mine) or overhanging pinnae shut out much of the light."

The native jungle palms of the humid Panamanian forest are species having pinnate leaves. The palm trees of which I write, however, are of a species introduced into many tropical regions from Fiji and nearby islands, and are commonly and, Dr. L. H. Bailey tells me, probably correctly called *Prichardia pacifica* Seem. & Wendl. This palm has great, stiff, palmate fronds.

Now the bats have begun to make use of the superior shade producing quality of these leaves through having developed a most ingenious habit.

The lower side of the leaves is stiff and smooth as can be and, of course, plicate; but by nipping the ridges of the plications on the under side the leaf is weakened and as the bitten spots are skillfully and serially distributed the leaf finally is sufficiently weakened so that the distal portion droops sharply downward. The bitten spots (fig. 3) offer an easy foothold and beneath the line of the fold is a shady daytime roost. Figure 2 and figure 3, more enlarged, show this extraordinary process better than a long description. In time the distal part of the leaf dries up, dies and sloughs off and a new leaf has to be cut. I saw one such new leaf almost finished and I have no doubt but that the bats were nibbling at the leaf with their

teeth and that the work is not done with their weak little feet as I at first supposed it might be. The initial nips must be made while the bats hover beneath the leaves as the untouched leaf can afford no chance for clinging until the fibres are torn to provide a foothold.

The little colonies under the leaves are most interesting to observe. They are decidedly active, even at noon-day when the torrid sun must make their roost a veritable Gehenna. They keep up a constant bickering, nipping one another, fanning themselves with partially spread wings and, if I mistake not, biting at the leaf from time to time. They move about so much that details are hard to see clearly.

FIG. 1. PALM TREE SHOWING LEAF CUTTING BY BATS

The colonies are by no means stable. The roadside tree at Pedro Miguel was visited day after day and the bats varied in number from 56 to 19. The tree on Mr. Bouchier's place, where I cut the "penca" figured (fig. 2–3–4) had that day but a single bat beneath it which Dr. Clark and I captured to check the identity. Mr. Bouchier told us that a few days before a considerable number of bats were there.

Clark and Humphrey have lived for years in tropical America and I have had good chances to see palm trees for I have spent a good part of many years in Central America. We have none of us ever seen this leaf cutting before. Did Goldman observe it (see the italicized quotation) but fail to recognize the cause of the folded leaf? It is equally probable that his observation was made when his bats of this species were taken under leaves naturally folded over, and they often are so taken.

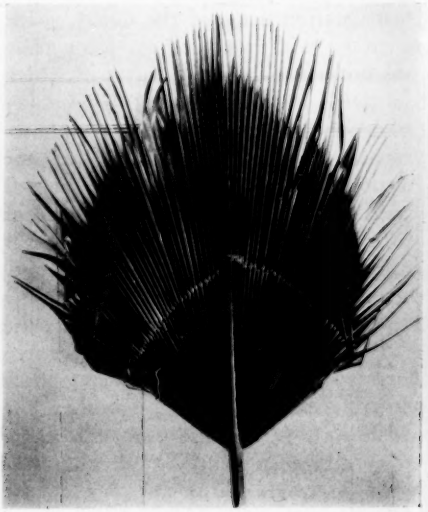

FIG. 2. PALM LEAF SHOWING LEAF CUTTING BY BATS

Now Clark has just (April, 1931) found for the first time a youngish coconut palm, a single leaf of which was being cut by bats of the genus Uroderma in a very similar way. This was at El Real de

Santa Maria on the Tuyra River in Darien, over a hundred miles from the Canal Zone.

IS THE HABIT NEWLY ACQUIRED?

I can find no mention of this habit in the literature. My colleagues know of none. Are we to suppose that this habit has been newly acquired to make use of an introduced fan palm which thus treated makes a better shelter than the native palms afford but which is otherwise useless, and is this leaf cutting habit spreading so that it is beginning to be applied to coconut palms? It is inconceivable that the habit should be a common one, else we and many others would have long since observed it. We have surely examined enough coconut palms, if only to put a bit of rum into a green nut, and be refreshed after the heat of the day!

FIG. 3. DETAIL OF LEAF SHOWN IN FIGURE 2

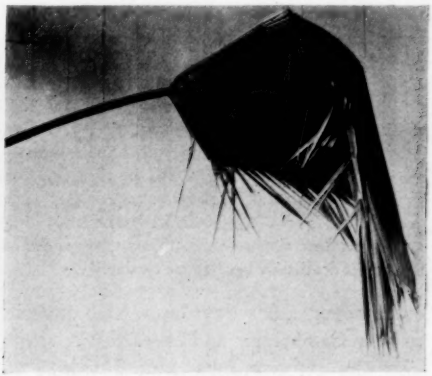

FIG. 4. DROOPING END OF PALM LEAF AS RESULT OF LEAF CUTTING BY BATS

Perhaps Uroderma acquired long ago, this leaf-cutting habit when the species was not confined to a region where the palms with pinnate leaves offered a reasonably satisfactory shelter, and then lost it, until the introduction of Prichardia "sprung the trigger" so that the leaf-cutting habit was revived. An easier answer would be to admit that the habit has always existed but has been unobserved. Uroderma, however, is not a rare bat and the observers have been many and diligent. The great question is—has this habit arisen *de novo* to meet a new problem and has this habit now begun to spread? I confess, I suspect that this may be the case.

After reading this little note to my friend, W. M. Wheeler, he called my attention to the curious parallel habit involving a somewhat similar symmetry in leaf-cutting which is shown by certain European beetles and which has been elaborately, and perhaps a bit over sympathetically, described by Father Wasmann in "Der Trichterwickler" a book published in Munich in 1884 (Aschendorff; p. i–vii, 1–266, text fig. and 2 pl.).

What I now await is the answer from someone who will show me that the habit really has often been recorded before and that it is as well known as some of the other "new discoveries" which we have heard heralded from the house tops only too often. The result will be that chastened feeling which we are told is excellent medicine for the soul.

ARE THE BATS RESPONSIBLE?

After writing the foregoing paragraphs I sent them to my friend, Mr. Gerrit S. Miller, in the United States National Museum, asking him if he had ever heard of bats doing any leaf cutting of this sort. He replied, "With amazement at the unconvincingness of the observations," and added:

> My memory fails to vibrate to your touch—I cannot recall having heard or read anything of the kind before. The circumstances that leave my skepticism intact are several. You do not report having seen the bats beginning their operations on a normal, unbent leaf that would offer no protection until after it became bent. You do not tell whether or not it is possible to find leaves bent presumably by other agencies, such, perhaps, as ants, wind or rain, and not inhabited by bat colonies. You say on page 3 that you "have no doubt but that the bats were nibbling at the leaf with their teeth," but you do not say that the 'nibbled' areas of the leaves show marks that correspond in size and in distance apart to the bats' canines. Finally you say, on this same page, "They keep . . . and if I mistake not, biting at the leaf from time to time. They move about so much that details are hard to see clearly."

However, a few days later, he happened to be in Cambridge, and I was able to show him the photographs and, I think, to convince him that neither ants, nor wind, nor rain had anything to do with the matter. His intelligently skeptical attitude set me to thinking and I remembered that Dr. Liberty H. Bailey, of Cornell, a most distinguished authority on the Palmae, was happily then at the Barro Colorado Island Laboratory. I wrote immediately and asked my friend, Mr. James Zetek, the Resident Custodian of the Station, to drive him at once to see the cut palms. Mr. Zetek writes that Dr. Bailey's decision is that:

> Every indication points to the fact that the injury is caused by the bats and he sees no morphological reason for breakages other than that. The proof would lie in making the observations at the very beginning of the injury when the bats start to cling, but Dr. Bailey was unable, of course, to do this. . . . All of the bat work was done to leaves of Prichardia. We saw several cases where leaves were not injured, and there was present a peculiar morphological crease at the base of the segments. Doctor Bailey had one leaf cut off to enable him to examine this peculiarity better, and later on at house 54 he was able to see a small palm which had these creases and also had the typical bat injury. The bat injury is not at the site of these creases but often considerably below them. Not all leaves show these creases. In some cases we found three rows of the typical bat injury on a single leaf.

"I," adds Zetek, a Government Entomologist, "see nothing at all which would point to insects causing the injury."

Mr. Zetek, however, then made a still more surprising discovery; this was that Mr. Peters, the District Quartermaster of the Pedro Miguel area, had been keeping a record of the palms showing these injuries, examinations having been made twice a week from January 15, 1931 to May 8, 1931. There are palms of this species on the premises of fifteen employees' quarters in and about Pedro Miguel. Some palms show as many as ten cut leaves but the extraordinary way in which the bats move from place to place bears out the observations which I made and which are recited earlier in this paper. The report of Mr. Peters' inspections is given herewith. It should be noted that the figures mean the number of leaves cut by bats. Figures in boldfaced type mean that bats were present at the time the inspection was made. Mr. Zetek, who copied this report for me, notes that in some cases the inspector had "X" instead of a number which means that he did not count the exact number of cut leaves found that day, so Mr. Zetek carries forward his previous count.

The case of No. 127 is extremely instructive.

This is all the evidence I have been

able to gather together regarding this most perplexing state of affairs. I did not need to be told by Mr. Miller that bats were not given to nest building or home making or similar domestic activities. This I knew perfectly well and I, of course, also know perfectly well that there may be some other explanation of the cutting of these leaves but until someone shows us what this may be I maintain that we have a pretty clear case in favor of the bats doing it and I hope, if I live, to return and try and find out more concerning this surprising state of affairs.

Number of palm leaves cut by bats

Inspections made 2 times a week at Pedro Miguel starting January 15, 1931; ending May 8, 1931

HOUSE NUMBER	JANUARY						FEBRUARY						MARCH								APRIL								MAY	
51	0	0	0	0	0	0	0	0	0	1	1	1	1	1	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
54	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
105	2	2	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
107	0	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
112R	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
112L	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
127	3	3	7	7	7	7	7	7	7	7	7	7	7	8	8	8	9	9	9	9	9	9	9	9	10	10	10	10	10	
141R	0	0	1	1	1	1	1	1	1	1	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
141L	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
151	2	2	4	4	4	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5
181R	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
181L	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3
183R	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
183L	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
221	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

NOTE ADDED APRIL 27, 1932

I returned to the Canal Zone early in March 1932 with the hope of finally settling the question of whether this leaf cutting was the work of bats or the making use of accidental leaf injuries or whether possibly insects or some other causative agent was involved.

After reaching the Isthmus I spent a week at Barro Colorado Island where I learned that Dr. Chapman had made a pertinent observation which will be the basis of a later report by him. I then proceeded to Ancon. Dr. R. K. Enders came over from the Island and joined me as another witness of what might be observed. Mr. Humphries, Dr. Clark and Mr. Peters, the District Quartermaster at Pedro Miguel, were all most interested and have helped me in every way.

There are sixteen specimens of Prichardia planted in the town limits of Pedro Miguel. Of these fourteen are sizable trees, two being small potted individuals. Of the fourteen large trees eleven had from one to eight of the leaves cut. On 3–12–32 four trees showed bats present at early dawn and five at 10:30 A.M. There were thirteen adults plus seven large young; one bat; twenty-two with several young, all under one leaf; one adult and young; two adults and one young; and two solitary bats under two leaves, and fifteen under another. Five trees with about fifty-six adults and perhaps a dozen young of varying ages. The following day three trees had twenty-one adults and five or six young. The rest had vanished.

Our next procedure was to remove every cut leaf from every tree. This was done during the morning of March 14, 1932.

The disturbed bats were most interesting to observe. The swift, accurate flight as

they hurried from tree to tree evidently knowing the whereabouts of every cut leaf, was astonishing to the last degree. I have never seen bats so ready to fly about in brilliant noon day sunlight, nor so utterly accurate in turning and twisting about. The bats were most obviously greatly disturbed at the unexpected impossibility of finding their wonted abodes. So we left them. A check up at late afternoon showed no change.

I quote from my notes.

> This morning, however, March 15th, I hurried to Pedro Miguel, Dr. Enders having gone to visit the bat caves in the Chillibrillo River valley. To my joy at house 54, a small vigorous palm which has shown a rather large colony consistently present was found to have a big fresh frond, upright fourteen hours before, completely nipped across its entire breadth, the distal portion fallen to form a perfect shelter and with eight bats in a cluster under the fold. Another tall palm in front of the Postoffice had a high leaf completely folded over and twenty-two bats in a dense clump resting in its shade. Another leaf, partly cut, sheltered a single bat.

Uroderma is peculiar not only in its ability to fly far and wide by day and often to change its abode, but in its alert nervous watchfulness. The pale runs to the ears and the facial stripes give it a peculiarly alert expression. Moreover, it has singularly weak but delicately agile feet. The bats as often as not do not roost by hanging to the cut and frayed zone, as I supposed last year but can and very often do hang in clusters above and below the cut zone. They move about a good deal, bickering and fanning with half opened wings through the whole day.

The cutting can be done with the teeth only and must be done for shelter alone and not because the cut abraded zone is necessary for successful roosting.

In the Canal Zone Experimental Gardens at Summit we found several trees of the fan palm *Livistona chinensis* with cut leaves and with colonies of Urodermas. But there are no Prichardias of suitable size at Summit. In Summit Dr. Enders and I shot a good many colonies and were led to conclude in general that the males may usually roost alone and the females in clusters. We found Uroderma with young *in utero*, with newly born young, and with young still being carried about which were nearly as large as the parent.

I quote again. "17th evening. Drove to Pedro Miguel, at eleven with spotlight; Enders, and Mr. and Mrs. Humphries.

"Bats flitting about palm at house 54 from which big cut leaf had been removed at 6 P.M. Bats very abundant and some evidence of slight erosion. Left when light used." In the morning the tree was abandoned. In front of the Postoffice some leaves were partially cut and found hanging but no new cutting had been done since the previous evening and but a very few bats were present in the morning. This colony had been a large one.

A few visits with the spotlight made it quite evident that no observing could be done and the bats' dislike for this intrusion was shown by their generally disappearing. When I last went the round at Pedro Miguel the bats were no more but to my surprise I saw several leaves cut on a Prichardia at Red Tank a mile at least away.

I sent some of the leaves to my doubting friend Miller and when I saw him in Washington a few days ago he admitted in moderation that it looked now a little as if I might be right after all. I rather think so too.

SEX DIFFERENCES IN MORTALITY IN ABRAXAS-TYPE SPECIES

By JOHN W. MacARTHUR and W. H. T. BAILLIE
University of Toronto

THEORIES AS TO THE CAUSES OF SEX DIFFERENTIAL MORTALITY

THE inequalities in the mortality and longevity of the sexes that are almost universally evident in organisms (Pelseneer, 26; Geiser, 8) have most frequently been ascribed to sex differences either in occupational risks, in body size, in metabolic activity or level, in chromosome balance, or in frequency of sex-linked recessive genes impairing vitality. The lack, or inconstancy from species to species, of characteristic sex differences in body size, habits, or occupational hazards, and the fact that such differences, where found, are comparatively small in very young individuals, which already show large mortality differences, appear to eliminate these as general or important causes of the unequal death rates. The field has therefore been left chiefly to theories emphasizing either the dissimilar chromosome equipment of the sexes or their different levels or types of metabolism.

The sex-linked lethal theory (Lenz, 22; Geiser, 8; Gunther, 14; Huxley, 20; Schirmer, 30) seeks to explain the higher male mortality on the view that the male with but one X chromosome would inevitably suffer more frequently from sex-linked recessive characters which, being often lethal, semi-lethal, or injurious, would tend to reduce average vigor and viability—than would the females with two X chromosomes. If such a character is present in $\frac{1}{n}$ of the males of a population it would be found in but $\frac{1}{n^2}$ of the females, and the rarer the character the greater would be the excess of males possessing it. Factors in any degree lethal should be more effective in eliminating the heterozygous sex, since any recessive genes carried in its single X chromosome cannot be masked by a dominant normal allelomorph. According to Lenz (22), lethal factors need occur only in one in 4,000 genes to produce the sex differences observed in human death rates.

Haldane (16) noted that, when in interspecific crosses one sex is abnormal, sterile, or deficient in numbers, that sex is usually—though not invariably—the heterogametic one (XY male or ZW female). Gowen's results (12) with measuring the average duration of life and the metabolism of *Drosophila* triploids and intersexes were also taken to support the view that chromosome imbalance as well as different metabolic rates may be a prominent cause of differences in longevity.

We pointed out briefly in a former paper (23) that proof of the sex-linked theory is faulty in many respects and particularly that its adherents, excepting Geiser, had hitherto neglected a most important line of evidence in failing to apply the crucial test of determining the relative mortality of the sexes in Abraxas-type forms where the male has the two sex chromosomes and the female but one. The occurrence in nature of the two opposing types of sex-

determination and sex-linkage, one based on male heterogamety, as in *Drosophila*, and the other on female heterogamety, as in *Abraxas*, offers a unique opportunity for estimating critically the importance of the sex-chromosome mechanism as such in producing, either by unequal distribution of genes or by imbalance of chromosomes, the sex differences in mortality. A peculiar interest thus attaches to an accurate objective determination of the relative viability of the sexes in birds, Lepidoptera, and certain Cyprinodont fishes, for these are animal species where the females would be expected to exhibit a higher mortality than the males, providing this is caused mainly or wholly by the phenotypic expression of recessive sex-linked deleterious factors, or by sex-chromosome imbalance.

Sex differences in mortality are most satisfactorily demonstrated by direct observations at frequent censuses throughout life of the deaths occurring in males and females of a large and representative group of individuals; from such data the mean and maximum duration of life of the sexes and any significant change in the sex ratio at advancing ages may be determined. Adequate or convincing sex mortality data are available for a considerable number of Drosophila-type species (man, a few other mammals, many fish, insects, crustaceans, arachnids, nematodes, molluscs, and dioecious plants), in nearly all of which the males show an inferiority in hardiness and viability (8, 26). Data as good, are, or have been, quite lacking for Abraxas-type forms, even for the species so widely multiplied in large scale commercial production or in extensive laboratory experiments, as fowls, pigeons, canaries, sword tails and platyfish, silkworms, *Abraxas*, *Colias*, and gypsy moth. The earlier information was at best fragmentary, based on opinions, impressions, estimates and more or less casual and incidental observations which vary greatly in statistical merit. More recently good evidence of a sex-differential mortality in certain birds and moths has been collected, but in several of the moth species studied female heterogamety must still be assumed from their taxonomic position, since direct cytological or genetic evidence is lacking.

SEX DIFFERENTIAL MORTALITY IN BIRDS

The earlier literature contains a few suggestions of interest here. Darwin's correspondents assured him (4) that male birds were caught in greater numbers than were females; he accepted the presumed preponderance of males in nature as a fact in support of his theory of sexual selection. Friedman found a conspicuous excess of males in the social parasites, such as the cowbirds. A review (26) of the sex ratios of wild birds collected by field naturalists and game hunters shows that, in the group as a whole, excepting Raptorial species, there is often a considerable overplus of males both at hatching and in the adult condition. But it would seem only natural that early-migrating, highly colored, singing and active males would be much more likely to be seen and taken than the females, which are usually tardier in their migrations, soberer-colored, more silent and retiring. Pelseneer's summary (26) closes with the final statement that "in the known cases where observation has been possible, the polyarrheny is more feeble in the adult state than in youth;" that is, the common early excess of males at hatching and in young birds may be preserved into adult life, but it becomes less marked owing to the shorter life and higher mortality of the males, and is often followed by an approximate equality of the sexes, or even by an ultimate numerical predominance of females.

A partial confirmation of this view is obtained in the extensive Zoological Gardens records of Gurney, and Flower. Checking all the cases from Flower's data where the sex of the longest-lived individual is stated it appears that in 24 species males attained the greater age, and females in 35 species. Such records are imperfect, however, since the age and condition at collection of the captive birds is somewhat uncertain at best, and unequal numbers of the sexes may have been taken. In two vanishing wild species, the passenger pigeon and the heath hen, the last surviving individuals have been males.

Doves and pigeons

In the Japanese turtle dove Whitman (31) reported that the males are oftener the longer-lived, but the numbers are so extremely meager that they afford no statistically adequate evidence of such a selective mortality. Darwin said (4): "In regard to domestic pigeons, there is good evidence either that the males are produced in excess, or that they live longer, for single males can always be purchased cheaper than females;" and that evidently "the hen is generally the weaker of the two, and more likely to perish." Cole and Kirkpatrick (2) did find a sex ratio of 105 males to 100 females in the pigeon, but their observations on an initial population of 1,676 birds continued for a period of over six years revealed a high but apparently very evenly balanced death rate in the sexes. They found 44 males and 37 females dead in the shell; from a total of 866 males and 810 females hatched, 343 males and 307 females in all died up to the twenty-eighth day, that is 39.61 per cent of the males and 37.9 per cent of the females. Total deaths up to six months were 478 males (55.19 per cent) and 449 females (55.43 per cent). Among 172 pairs of squabs dying before six months, the male died first in 88 cases, the female first in 84 cases. An early period with an excess of male deaths was followed by the adolescent and reproductive periods with a slight excess of female deaths, but, on the whole, there was no significant change of sex ratio with age. The problem of a sexually differential mortality in this species is thus still unsettled.

Pheasants

In Haig-Thomas' (15) extensive material from pheasant species crosses as worked over by Huxley there was not only a predominance of males hatched, as is usual in avian species crosses, but also a large and distinct excess of male deaths both before and after hatching. Among 514 male chicks hatched 70.43 per cent died within six months, and of 252 female chicks only 50.79 per cent failed to reach that age. A secondary sex ratio of 67.1 per cent males thus was reduced to one of 55.1 per cent males in the adult state. The mortality undoubtedly caused a strong selective elimination of males; "it is abundantly clear that the mortality of males is much greater than that of females in the early period of life" (15), a fact which Huxley sought to connect with a supposed sex reversal. This case throws support to the view that birds do *not* necessarily show "a reversal of the survival relations of the sexes found among animals possessing the other type of genetic constitution." (8).

The common fowl

In his alcoholization experiment Pearl (24) found in the progeny of both control and treated parents, that in general the females showed "a greater proportional mortality." Twenty-three per cent of the 413 male and 30 per cent of the 463

female chicks (listed in his tables 5 and 6) died in the first 180 days under the drastic conditions of the free range. But the difference between the sex ratio of chicks banded and of those dying is only about two probable errors; further, all chicks that were "obviously malformed" or "lacking in vitality" were rejected from the experiments, and these may, and in our experience probably do, include a very high proportion of males. This may help account for the very different results obtained in the following two instances.

For growth and genetic studies (1926–1931) over a thousand chicks of several pure breeds (Barred and Columbian Rocks, Brown Leghorns and Campines) and their F_1 and F_2 hybrids were taken unselected and kept under our close observation, and the sex of all chicks dying within the first 120 days was determined by dissection (table 1).

TABLE 1

Showing for the common fowl the percentage mortality during the first 120 days and the average age at death of chicks dying in that interval

	MALES				FEMALES			
	Number	Deaths	Deaths	Average age at death	Number	Deaths	Deaths	Average age at death
			per cent				*per cent*	
Pure breeds	203	58	28.57	13.64	187	56	30.0	13.70
F_1 hybrids	191	41	21.47	14.70	186	32	17.2	18.79
F_2 hybrids	133	49	36.84	15.39	130	37	28.85	20.78
Total	527	148	28.08	14.50	503	125	24.85	17.10

In this test the males clearly tend to die in greater proportions and at an earlier age than do the females. The sex ratio obtaining in the hatches producing the 1,030 chicks banded was 51.16 ± 1.05 per cent males; in the 273 dying within the first 120 days the sex ratio was 54.21 ± 1.92 per cent males. After this heavy reduction of the males the sex ratio of the survivors was necessarily lowered—to 50.06 ± 1.32 per cent. The difference in the male ratio in the original population and in the lot dying early, being 3.05 ± 2.19 per cent, is not actually significant statistically, but inclines to support the view that the early mortality in chicks is somewhat sexually selective by discriminating against male survival and tending to raise the proportion of females in older chicks.

The more extensive and really significant data of Landauer and Landauer (21) seem finally to prove the above contention. These authors reported on the deaths occurring in the first eight weeks among 19,100 crossbred and Leghorn chicks hatched from 1922 to 1930 at the Storrs station. They observed a total of 2,996 male and 2,687 female deaths, a ratio of 52.7 ± 0.45 per cent males, a deviation from equality of six times the probable error. They conclude that "in chickens as in man the postnatal mortality is higher in males than in females." The observed low sex ratio at hatching, 48.77 ± 0.13 per cent males, again suggests that "if any differential mortality occurs during embryonic development it is the male sex which suffers more" (21). Hutt found that monstrosities and malpositions cause much of chick mortality during incubation (19), and his earlier

studies indicate that, though neither sex is heavily discriminated against, males may suffer rather more than females from at least some types of teratisms, e.g. chondrodystrophy. Practically nothing is known of the relative death rates in older cocks and hens.

In general it is difficult now to justify Geiser's statements that "studies in mortality in birds have demonstrated the greater longevity of the males" and "the differential death rate of the sexes in fowls operate to produce the familiar preponderance of males in flocks of young birds" (8). His application of the sex-linked lethal theory to birds was logical, but was built up on a foundation of observations of debatable import; the failure of its factual support leaves the theory untenable, and the onus of providing new supporting data would clearly seem to rest with its adherents. At least in the fowl, both pure and crossbred, and in the hybrid pheasants, the young males do not exhibit that greater viability which would be expected if sex-linked recessives were the chief cause of the sex differences in mortality.

The published works—of Bellamy, Fraser, and Gordon—apparently do not supply data, which would be especially interesting to have, on the relative longevity of the sexes of the Abraxas-type fishes, *Xiphophorus helleri* and the *Platypoecilus* races.

SEX DIFFERENTIAL MORTALITY IN LEPIDOPTERA

Weismann cites the case of the butterfly, *Aglia tau*, where the males appear to outlive the females. Collectors and expert observers like Bates and Wallace assured Darwin (4) that among wild butterflies and moths, as among birds, males were caught in greater numbers, were cheaper, and lived better than females. It was considered "strange" and "inexplicable" if adult females were as numerous as the males. Such general information as to the numerical proportions of the sexes is subject to the same objections as in birds, and affords no reliable evidence of the mortality rates actually obtaining in this group. Darwin's special emphasis on the excess of males in adult birds and Lepidoptera might be taken to indicate a recognition of a fundamental difference between these groups and other animals, since shown in the sex-determining mechanism, but evidently he was misled by the untrustworthy

TABLE 2

Showing the average longevity in days of the imagoes of moths (data from Rau and Rau (7, 8) for the first six species)

SPECIES	MALES		FEMALES		MALE: FEMALE LONGEVITY RATIO
	Number	Mean duration of life	Number	Mean duration of life	
Samia cecropia	744	10.42	709	10.06	103.6 :100
Samia californica	6	8.67	6	8.84	98.08:100
Tropaea luna	35	5.86	25	5.96	98.33:100
Philosamia cynthia	204	5.94	155	7.11	83.54:100
Telea polyphemus	399	8.06	338	10.00	80.60:100
Callosamia promethea	162	4.57	112	7.04	63.49:100
Pyrausta nubilalis		13.0		17.4	74.7 :100
Pyrausta penitalis	27	6.77	43	7.72	87.7 :100

impressions and estimates of his correspondents.

The series of papers by Rau and Rau (27) dealing with large saturnid moths are especially thorough and critical; and their findings have often been cited (8) as supporting the contention that in this group males commonly outlive females, but the totalled figures as reported definitely indicate the contrary for five out of six of the species (table 2).

Male imagoes do often outnumber the females, as other collectors stated, but the cecropia moth is the exception in that the male moths outlive the females. When pairs of the corn-borer moth, *Pyrausta nubilalis*, were confined in oviposition cages with a supply of water the life of the males was definitely shorter than that of the females (Crawford and Spencer, 3); the same is true for the related lotus-borer, *Pyrausta penitalis* (Ainslee and Cartwright, 1). Graf's observations (13) on isolated pairs of the potato-tuber moth, *Phthorimea operculella*, showed that the males died first in 221 of the 275 pairs and "prove that the length of life of males is less than that of the females." Farkas (6) found that male silkworm moths died earlier than the females. In many Lepidoptera, as the gypsy moth, the males are known to be so precocious and short-lived that they are often gone before the later females are ripe (Goldschmidt, 10). Kopec (26) found that when gypsy moths were starved the males lived a maximum of eight hours, the females as long as ten to fourteen hours. Gerould (9), from a long and intimate experience in breeding butterflies of the *Colias* species, writes that "the males have a shorter imaginal life than the females, whether well-fed or not fed at all."

Thus, with a single exception, all of the twelve species of Lepidoptera for which we have found more or less adequate direct data show the usual excessive male mortality; and another good case may now be added to this list.

MORTALITY IN THE CODLING MOTH

Especially full and useful information on the mortality of the sexes of the codling moth, *Carpocapsa pomonella*, is contained in publications of the United States Department of Agriculture (31). The studies were made with evident care, on a large scale, and repeated over several years and in stations widely separated covering all sections of continental United States. They deal, moreover, with all stages of the life history and with all generations of the pest that occur in the several regions. Both the number of generations produced annually and the average duration of life are clearly related to the temperature belts in the climatic gradation; in the most southerly station in Pecos Valley three or even four short-lived generations occur, while at more northerly and more elevated sites three, two, and finally but a single very long-lived generation is completed.

In general the most useful data from this source include the records of longevity of imagoes from daily observations of large numbers of mated moths reared in their natural sex-ratios—45.4 ± .17 per cent males in 40,477 moths tabulated—in large outdoor oviposition cages (table 3).

It is evident that at all stations and in all generations, excepting only the first broods in Colorado and Washington, the males are the shorter lived. The weighted average duration of life of females (10.23 days) exceeds that of males (9.40 days) by 0.83 day, a difference of over twenty-five probable errors, and highly significant in view of the large number on which it is based.

The more recent evidence thus practically demonstrates that the birds and

Lepidoptera, considered as groups, afford no exception to what seems to be a "very general zoological phenomenon" (26); the larger destruction and shorter life of males causes them to become relatively fewer at advanced ages. The initial high male ratio often found may be considered adaptive as it compensates for the short life and high mortality of this sex (26, 17).

If the data for the codling moth in the various stations be arranged in order of increasing average duration of life of the males it becomes clear that the differential mortality between the sexes is most marked in southern stations, and least evident in colder regions (tables 4 and 5).

TABLE 3

Showing the average duration of life of males and females of the codling moth observed in several stations in the United States

PLACE	YEAR	BROOD	NUMBER OF MOTHS		MEAN DURATION OF LIFE IN DAYS	
			♂	♀	♂	♀
New Mexico	1912	spring	325	373	6.7	8.47
(Pecos)		first	367	411	4.44	6.24
		second	659	757	5.49	7.58
(Mesilla)	1914–19	all	8,009	9,657	6.0	6.9
Virginia	1922	spring	276	337	8.87	11.49
Arkansas	1921	spring	100	86	9.28	12.69
Michigan	1911	spring	153	177	9.18	10.63
		first	456	563	9.57	11.49
Pennsylvania	1909	first	57	92	9.79	11.47
Maine	1913–14	spring	263	295	11.51	12.56
Colorado	1915–16	spring	2,554	2,919	14.69	15.79
		first	2,330	2,620	12.70	12.35
Washington	1919–21	spring	1,148	1,478	14.44	15.67
		first	1,302	1,883	13.24	12.55
Total			17,999	21,648	9.40	10.23

INFLUENCE OF CONDITIONS ON THE RELATIVE LENGTH OF LIFE OF MALES AND FEMALES

There is much evidence showing that the sex differential mortality is not fixed and constant, even for a single species, but that it varies greatly with external conditions, age, type of breeding, etc.

There is much to suggest that temperature is the most influential external condition in this case. In higher latitudes and elevations, and in the colder season of the year in New Mexico, the life span is lengthened, but the sex difference is lessened. Constants calculated from the original data of table 4 show that the difference between male and female length of life represents 34.2 probable errors in New Mexico, 11.0 P.E. in Michigan, 3.0

TABLE 4

Showing the decline in sex-differential mortality in the codling moth in passing from southern to northern stations

AGE IN DAYS	NUMBER OF SURVIVORS UP TO INDICATED AGE							
	New Mexico		Michigan		Maine		Washington	
	♂	♀	♂	♀	♂	♀	♂	♀
0	1,000	1,000	1,000	1,000	1,000	1,000	1,000	1,000
1	998	1,000	999	999	1,000	993	998	998
2	976	989	982	989	985	990	991	993
3	876	960	952	976	954	973	977	981
4	693	892	896	947	935	959	953	965
5	439	773	842	908	878	915	926	941
6	238	593	755	851	810	858	885	905
7	135	419	657	797	722	790	822	848
8	70	278	548	689	635	736	756	796
9	44	188	442	600	510	647	692	735
10	24	119	348	499	433	566	618	662
11	11	79	281	416	403	492	563	595
12	5	53	204	339	357	431	515	536
13	3	39	172	281	316	386	461	476
14	2	24	115	224	266	298	411	428
15	2	14	82	172	240	251	356	369
16	2	10	59	135	198	217	309	318
17	2	6	36	109	160	183	264	271
18	1	5	25	86	144	153	229	221
19	1	3	21	69	106	143	201	188
20	0	1	13	53	80	112	169	155
21		1	10	34	65	92	138	122
22		1	8	22	53	81	114	98
23		1	5	18	42	64	98	81
24		0	5	15	38	51	82	62
25			3	15	34	41	68	48
26			2	10	34	37	52	35
27			2	10	23	31	40	24
28			2	8	23	17	32	16
29			2	5	23	10	23	12
30			2	4	19	10	21	10
31			2	4	11	10	18	6
32			0	3	8	3	15	6
33				1	8	3	13	5
34				1	0	3	11	3
35				1		3	9	2
36				1		3	7	2
37				0		3	6	2
38						0	5	2
39							3	1
40							3	1
41							2	1
42							2	1
43							1	1
44							1	1
45							0	1
50								0
Absolute number of moths.....	1,361	1,561	609	740	263	295	2,450	3,361

P.E. in Maine, and only 0.7 P.E. in Washington.

Rau's experiments (27) seem to show a very similar effect of low temperatures on the longevity of saturnid moths; under cool conditions the life of male cecropias was lengthened five per cent more than the females', that of male polyphemus moths ten per cent more.

The experimental evidence from the crustacean, *Daphnia magna*, likewise showed that the mean duration of life of females exceeded that of males by 33.35 per cent at 28°C., by 15.82 per cent at 18°C., and by only 0.43 per cent at 8°C., the male temperature coefficient throughout averaging fifteen per cent higher than the female. It was demonstrated that this extreme response of the males was a factor of their higher metabolic level (23). The basal metabolism of male pigeons was found more easily and extremely affected by unfavorable conditions, such as an unusually high temperature and low oxygen supply (29).

There are many further experimental proofs that males, whether XY or ZZ in constitution, are less resistant than females to various adverse conditions. When dioecious species extend to their northern limits of tolerance, the males are often scanty or lacking in the northern border of the range (26). In fishes, *Xiphophorus* males were 2 to 25 per cent more susceptible to KCN, alcohol and extremes of temperature (5); *Gambusia* males suffered a distinctly greater percentage loss by deaths in long shipments, especially in the summer season, and by direct test were much less resistant to high temperature, H-ion, KCN, and deficiency of oxygen (17). Among insects, male house-flies and ants are more susceptible to toxic agents, and males in *Drosophila* and *Lymantria* are less resistant to starvation (26). In general more males than females are found parasitized (26). There is evidence, not unequivocal, that inbreeding, e.g. in the fowl, especially lowers the vigor of the males; and it often lowers the male sex-ratio (20).

The general observations on man have shown that adverse conditions (crowding by multiple fetuses, abortion, still-birth, illegitimacy) seem to intensify, and favorable conditions to neutralize, the differential male mortality during pregnancy (20); improving external conditions, however, has raised the ratio of male to female deaths, by making the elimination more selective (18).

TABLE 5

Showing the amount of the sex-differential mortality in the codling moth

	AVERAGE LENGTH OF LIFE IN DAYS		LONGEVITY RATIO MALE : FEMALE	PERCENTAGE DIFFERENCE IN MORTALITY FEMALE > MALE
	♂	♀		
Pecos valley	5.50	7.45	73.83 : 100	35.45
Virginia	8.87	11.49	77.20 : 100	29.31
Arkansas	9.28	12.69	73.13 : 100	36.74
Michigan	9.51	11.28	84.31 : 100	18.66
Pennsylvania	9.79	11.47	85.35 : 100	16.95
Maine	11.51	12.56	91.64 : 100	9.1
Colorado	13.74	14.18	96.80 : 100	3.2
Yakima valley	13.84	13.92	99.43 : 100	0.58

DISCUSSION

The *sex-linked lethal theory* has in its favor that it accords well with Haldane's rule and with a sex-determining mechanism which in several Drosophila-type species has been demonstrated usually to eliminate more males than females. The lowered vitality of the males of certain mutants in *Drosophila* is undoubtedly due to deleterious recessive sex-linked genes; largely on this basis, and perhaps owing to the preponderating influence of work on *Drosophila* upon the development of genetics in general, it has been assumed and even asserted positively and finally (20, 22, 30) that the inequalities in mortality in man, and presumably in normal wild flies and in most other species (8), is caused in a similar mechanical manner. It would be the logical and attractive view to adopt if it could be extended safely and consistently also to the Abraxas-type species.

But the evidence collected furnishes many objections to this theory (most of which apply similarly to the allied chromosome imbalance theory):

1. The reversal of the sex mortality relations expected and predicted in avian and lepidopteran forms on this theory is apparently not found; according to the evidence cited, which seems the best available and fairly conclusive, these groups are not characterized by any peculiarly high male secondary sex ratios, and the ZW females are not like XY males in showing a higher mortality. That is, the possession of a duplex condition in the sex chromosomes does not appear to be directly correlated with a greater longevity in the homogametic sex. The general explanatory value of the theory is thus severely limited since it is applicable to only a selected part of the cases, and cannot cover the whole field consistently. It is inadequate as a generalization and the theory must at least be heavily supplemented by other explanations to account for the facts.

2. It is not clear that the theory even explains the mortality relations in Drosophila-type forms. The magnitude of the sex differences in mortality should in general, but apparently does not, agree quantitatively with a theoretical expected frequency of sex-linked genes. Considering the relative number and size of the sex chromosomes, one would anticipate less sex-linked genes, and less sex differential mortality, in man than in *Drosophila*, and in both much less than, for instance, in Hymenoptera, where the haploid males may receive unfavorable recessive factors by sex-linked inheritance in each pair of chromosomes. Actually—from the admittedly insufficient data—the species with comparatively few sex-linked loci (many chromosomes, or small X chromosomes) seem to be as likely to show the same excessive male mortality as those with many sex-linked loci (few chromosomes, large X chromosomes, or haploid males).

Again, in man, Beeton and Pearson's statistical studies (25) show no higher correlation in length of life between mothers and sons (who would all receive sex-linked recessive genes if the mother manifested them), than between fathers and sons (who receive no sex-linked genes from the father). Yuan (33) also found that "The hereditary influence upon longevity (of sons attaining at least 20 years of age) is about the same from the father or the mother," though the mothers' expectancy of life at 30 years was 2.3 years greater than the fathers'. Human males (18, 25) suffer a higher morbidity and mortality rate than females, from the great majority of defects and of diseases, whether predominantly infectious or constitutional, prenatal or postnatal, en-

vironmental or hereditary, dominant or recessive, autosomal or sex-linked. Mortality from most specific causes appears to be in some degree sexually selective, the female proving on the average the more perfect and the more resistant. Shirmer (30) argues, however, that all such mortality differences can be explained by assuming that sex-linked genes affect the whole constitutional reaction of the sexes, in which case their effects must be assumed to be predominantly favorable in Abraxas-type species, but unfavorable in Drosophila-type species.

It may even be questioned whether the occasional occurrence of unfavorable recessive mutations as such has been shown to be the cause of most natural mortality in general. Wild races of *Drosophila* were found by Stock quite free from expressed lethals, unless inbred; yet they show without inbreeding the usual sex difference in longevity. Would wholly sound stocks show no sex differences in mortality? Prolonged close inbreeding renders a population essentially homozygous for all characters. Do sex differences in mortality disappear in long-inbred strains of rats, guinea pigs or *Drosophila*?

The mortality relations of the sexes are very variable; certain conditions and genotypes may cause the males to live longer than the females. Early observers of man and all the first experimenters with *Drosophila* reported the males as longer lived. Some recessive II-chromosome genes in *Drosophila* shorten, others undoubtedly lengthen the average duration of life; but, whether they shorten or lengthen the span, certain of these non-sex-linked genes completely reverse the usual mortality relations of the sexes and allow the males to outlive the females by a very significant number of days (11). Such cases suggest that mortality is controlled through the whole genetic and environmental complex.

There is some support of a rather vague and general sort for the rival *metabolic theory* (23). The sexes are characterized quite generally by different types and rates of metabolism (28). In a great number of species, whatever their chromosomal sex-determining mechanism (as man, rat, fowl, pigeon, silkworm (6), *Daphnia* (23), *Drosophila* (12)) direct or indirect evidence shows that the males have the higher basal and ordinary functional metabolic level, the difference apparently arising or existing early in development (28). In *Daphnia magna* the length of life of the sexes, and of either sex at different temperatures, was closely associated with metabolic rates (23). In *Drosophila* (12) and a number of other species (25) high metabolic activity and short life are somewhat related, and Rubner's law seems to have partial validity.

If for some reason mortality runs somewhat parallel with metabolic level, then avian and lepidopteran species would not behave very differently from other animals. The metabolic theory, or indeed almost any other general physiological explanation, would thus be much more widely applicable than any particular chromosomal theory, since in both XY and ZZ males the basal level is evidently under ordinary conditions higher than in the females.

The sex difference in mortality would then be considered more as a normal and primary sex difference, perhaps a "natural and constitutional peculiarity due to sex alone" (4), and the low viability of the male in general "just an incidental effect of his maleness" (18). The lethal mechanism would thus operate indirectly through sex and physiologically rather than directly and mechanically.

It must be admitted that little is known

as to the exact relations existing between metabolic level and resistance and hardiness in higher animals, or as to the effect on metabolism of specific mutant genes affecting viability and vigor. But metabolism is readily changed by such conditions as affect the sex-differential mortality, and it is not unreasonable to suppose that a male, with more intense physiological activity, more rapid expenditure of energy and a stronger and more sensitive response to stimuli, would probably prove less stable and less resistant as a mechanism, be more easily disturbed and upset, and more frequently eliminated by unfavorable environments, both external and internal.

CONCLUSIONS

The information available tends strongly to refute the theory most widely held that unfavorable recessive sex-linked genes reducing viability are the main or sole cause of sex differences in mortality. The most satisfactory older and the newly collected data indicate that, at least in the common fowl and pheasants among birds and in twelve out of thirteen species of Lepidoptera, the males, which are known or presumed to be homozygous for sex and would be expected on the theory to be longer lived, are actually deficient in viability or shorter lived than the females. As a general rule the males of all groups of animals, except possibly some from interspecific crosses, appear to show a similar low viability, irrespective of their homogamety or heterogamety. This situation is, again, hardly consistent with the implications of the chromosome imbalance theory.

The relative importance of injurious sex-linked factors has probably been overrated. Conceding to them a real influence one may hold their rôle to be minor and secondary, superimposed upon a metabolic or other control, and tending, especially in interspecific crosses and in mutant and inbred races rather than in normal wild stocks, to accentuate slightly the already higher death rate of XY males and to diminish somewhat that of ZZ males. The latter influence may be marked enough to produce a more evenly balanced mortality of the sexes in some Abraxas-type species.

The differences in the death rates of the sexes appear to be much more definitely, constantly, and intimately correlated with sex itself than with any particular nuclear equipment of sex chromosomes. Since both ZZ and XY males are known as a general rule to be characterized by a higher basal metabolic level than females, a metabolic theory of the sex differential mortality would be more universally applicable and has some other support.

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THE SENSITIVITY OF FISHES TO SOUND AND TO OTHER MECHANICAL STIMULATION

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WHAT changes of a mechanical nature in the environment of the fish act as stimuli? The literature on this problem, both theoretical and experimental, is most confusing, being replete with claims, denials and reiteration. On the theoretical side this confusion is largely verbal. Certain writers apply the terms hearing and feeling to the processes in the fish initiated by stimuli which would arouse processes so termed in the human subject. Others use the term hearing to apply to ear-reception, and feeling to apply to integumental-reception. This distinction disregards the nature of the stimuli. On the experimental side the confusion is largely the result of unsatisfactory methods. This point will be taken up in detail below.

Much more interesting to us than further debate as to what should be called hearing, and what feeling, is consideration of the experimental literature with a view to answering, as well as we can, these two questions: To what kinds of mechanical changes in the environment do fishes respond? What organs are involved in the reception of these stimuli?

TACTUAL STIMULATION

There is no doubt but that fishes respond to prodding, stroking and the like, no matter to what part of the integument these stimuli are applied. There is some reason, however, to suppose that not all parts are equally sensitive. The mouth and head regions appear to be the most sensitive in many fishes. Perhaps it is because of the general agreement that fishes do respond to tactual stimuli that very little experimental work has been done which might throw light upon the type of response elicited and upon the discrimination capacity of the animal. According to casual observations such stimuli may arouse a negative reaction such as withdrawal, or a positive reaction such as feeding or spawning.

Baglioni (1), studying the light-pressure sense of Balistes, stimulated various parts of the animal with hairs and threads and came to the conclusion that it reacted to lower intensities of stimulation than did the human. This conclusion was not based on quantitative data.

The best piece of work bearing on this topic is that of Maxwell on the shovel-nosed ray, *Rhinobatus productus*, (29), and the dogfish, *Mustelus Californicus*, (30). He worked with the fishes out of water fastened to a board. They were kept alive by passing a stream of water through the respiratory apparatus. Stroking the ray on various parts of the nose resulted in coördinated eye and fin movements. The eye movements were neatly recorded on a kymograph. Maxwell was interested in Loeb's statement that orientation in space is largely due to three groups of tropistic influences: those of light, gravity and contact. The first two have been made the subject of frequent investigation. Maxwell reports that the responses to these mechanical stimuli

were remarkably uniform. A stroke of a given intensity at a given point always aroused the same response providing other stimulation remained constant. This was found to be true both in normal fishes and in those whose forebrains had been destroyed. Maxwell found that a gentle stroking on certain parts of the head aroused a positive fin response, that scratching of these same parts aroused a decidedly negative response. Thus the fish discriminated between these two forms of tactual stimulation.

Observation of the sea robin, Prionotus, suggests that this animal possesses rather acute tactual discrimination, especially for objects touching its pectoral fins. It apparently senses food by means of these appendages. Morphological investigation indicates that only tactual, not gustatory, nerve endings are involved (13) (32).

Schiche (45) claims that in the catfish, *Amiurus nebulosus*, the barbels (which observation has shown to be involved in the selection of food) are not responsive to gentle tactual stimulation, but only to pinching. It seems rather likely that the failure of his fishes to give overt responses to touch was due to their being very tame. Had they not been tame it is probable that they would have responded negatively to the visual stimulation which must have accompanied the presentation of the tactual stimulus.

Most fishes avoid contact with either still or moving objects (except during the "love play" which sometimes accompanies reproduction). Certain species, however, and in particular the ground-feeders, prefer to maintain tactual contact with some part of their environment. Schiche observed that catfishes not only preferred contact but preferred the maximum contact. Thus he noted that they usually took positions in the corners of the aquarium. For the same reason, apparently, they preferred a sandy to a stony bottom. He found that a hollow glass or stone tube appeared to be considered by them the ideal resting place. The tendency of these fishes to maintain maximum contact was, however, subordinate to certain other tendencies, in particular those related to light.

When the intensity of mechanical stimulation is sufficiently increased, there results destruction of the integument and perhaps deeper injury. The so-called pain end-organs are then stimulated. There is no experimental study on the response of fishes to such stimuli. That the sensitivity of fishes to intense mechanical stimulation is not great seems indicated by common observation, an example of which may be given. "The Greenland shark when feeding on the carcass of a whale allows itself to be repeatedly stabbed in the head without abandoning its prey" (11).

WATER STREAMS AS STIMULI

Another type of mechanical disturbance is the flowing of water currents. The orientation of fishes in moving water with head upstream (so-called rheotropism) has been interpreted as a positive response to pressure. To Verworn (47), not merely "rheotropism" but "stereotropism" and "geotropism" were all responses to pressure. Parker and Van Heusen (40), and Jordan (16) have described the behavior of fishes in aquaria into which there flowed swift streams of water. Catfishes were observed by Parker and Van Heusen to give what might be interpreted as a positive response to pressure, since they took positions heading directly into the current. This is the behavior observed in most fishes under such conditions. The fishes observed by Jordan were hamlets (*Epinephelus striatus*

Block). These oriented with tail toward the source of the current 67 per cent of the time and laterally 32 per cent of the time, very rarely giving the so-called positive response to pressure. Apparently these fishes orient so as to protect the head region from stimulation by the current. Jordan measured the reaction time to a stream of water flowing at a rate of 1/28 liter per second through an opening with a bore of one centimeter. It was presented at "close range." The reaction time varied according to the parts stimulated: lip region, 7 seconds; caudal fin, 16 seconds; dorsal fin, posterior part, 22 seconds; cheek and operculum, 25 seconds; side of body, 30 seconds. Unfortunately, Jordan does not state the number of subjects used nor the number of trials given. Furthermore he does not state whether he controlled the order in which the various parts were stimulated, a factor which would unquestionably affect the results.

A fish may select one current rather than another on the basis of its speed of flow. Chidester (7) found that under normal conditions *Fundulus heteroclitus* selects sea water in preference to fresh water or to water containing various toxic and non-toxic chemicals. But this fish could be enticed into fresh water, double sea water and even M/10 solution of KCl in preference to sea water merely by increasing the pressure of the flow of the former to double the pressure of the latter.

LOW FREQUENCY VIBRATION

Between single or discrete changes in pressure of the surrounding medium, and the rapid repetition of such changes (sound) there lies a form of stimulation which may be termed low frequency vibration. Parker and Van Heusen, whose work will be taken up in detail in later sections, have noted the response of fishes to disturbances having a frequency of 4 to 5 per second. The fact that blind fishes successfully avoid obstacles, and that blinded fish soon learn to do so, has been taken to indicate sensitivity to stimuli of this kind. As a fish approaches an obstacle the water disturbance caused by its own motion and other water movement is supposedly reflected from the obstacle and the fish, detecting this low frequency "echo," avoids the object. The exact characteristics of this stimulus have not yet been determined. The vibrations are, in all probability, of lower frequency than those spoken of as sound. Crozier (8), while trying to study the "common chemical sense" of a blinded hamlet, *Epinephelus striatus*, by squirting chemicals at this well-scaled teleost, noted that the mere approach of the pipette caused the blinded fish to make an avoiding reaction. Further investigation showed that even though a glass rod were moved slowly and carefully toward the fish the response would be given when the rod was still 4 or 5 centimeters distant and would be of such a nature as to indicate that the rod had been accurately located. Crozier states that "mechanical deformations of the water of very minute amplitude and of a somewhat irregular nature are the sources of stimulation." This delicate form of stimulation aroused a response even though the water in the neighborhood of the fish was turbulent. The order of sensitivity (from greatest to least) of various parts seemed to be 1) tip of snout, 2) side of head, 3) caudal peduncle, 4) top of head, 5) side of body, 6) various fins.

SOUND

Investigators using the stimulus-test method

Do fishes respond to mechanical vibrations of high frequency or, in other words,

to sound? A large number of investigators have attempted to answer this question. Negative conclusions have been reached about as frequently as affirmative. Only the more important reports need be considered here.

Kreidl (21) was one of the earliest investigators to decide that under normal conditions the fish does not hear. He used sounds produced in the air by a bell, a whistle, clapping of the hands, and a pistol shot. He also used the sound produced under water by the vibration of metal rods. To none of these did the goldfish, *Carassius auratus*, make any response under normal conditions, although it did respond to the handclapping and the pistol shot after the administration of strychnine. A smart blow applied to the aquarium caused even the normal fishes to respond. The blow probably produced gross mechanical disturbance of the water in addition to sound and the fishes may have reacted to that. Kreidl was strengthened in his conclusion the following year (22) when he studied the behavior of trout raised in a market place at Krems. These were said to respond to the ringing of a bell by assembling at the surface to be fed. He found that when the bell was rung by a concealed person there was no response. His conclusion was that the stimuli causing the assembling were chiefly visual. Perhaps there was also gross mechanical disturbance caused by the treading on the bank.

Lee (25) reached similar conclusions, since he was able to detect response only to gross mechanical disturbance and not to handclapping, the human voice, nor to stones struck together either above or below the water. Körner (17) failed to see a response to sounds produced under water even though he tested some 25 different species. Later (18) (19), because other investigators reported results contradictory to his, especially as these related to the catfish, he tested this species again. This work corroborated, at least to his own satisfaction, his earlier investigation. Brüning (5) found that sticklebacks were undisturbed by the clapping of hands close above the surface of the water and by the human voice. He states that fishes in a pond were disturbed by treading on the bank.

Marage (28) reports an investigation using a rather novel type of stimulation. Synthetic vowel sounds were transmitted from the air to the water by means of a rubber tube closed at the lower end by a thin rubber diaphragm. None of the seven species studied gave evidence of hearing. Maier (26) studied 11 species of marine fishes and 12 species of fresh water forms, using various sounds including whistles and a submerged electric bell. None but the catfish gave any response and that form responded only to a shrill whistle. Bernoulli (3) used the same type of stimulation and also the shooting of a pistol. He obtained no evidence for hearing. Haempel (12) detected no response in Salmonidae or Cyprinidae to a shrill whistle or to a submerged bell but found that catfishes did respond.

Du Bois (9) has carried out an interesting investigation upon various fishes in their natural environment. They were observed through the glass bottom of a boat. The source of stimulation was the electromagnetically produced vibration of steel plates, 45 centimeters in diameter and 6 millimeters thick. The frequency was 1,000 vibrations per second. He says that the sound was high and squeaky, being easily audible and indeed very unpleasant as received by the human ear under water at a distance as great as five meters. By means of bait, fishes were

enticed to within a few centimeters of the plates, which were then set into vibration. In no case was any fish or group of fishes seen to make a response.

Kuroda (23) ingeniously contrived to record the gill movements of fishes on a kymograph drum. Thus accurate measurement of permanent records was substituted for the interpretation of movements by an observer. He used Carassius and Cyprinus, studying six individuals in all. No effect upon the breathing rate was produced by the sounding of electric bells in the air or by the sounding of a partially submerged bell. When the latter was vibrated so close to the fish that the wavelets produced reached the animal, it did give a response but this was a movement of the pectoral fin, not a change in the rhythm of the gill movement. The suggestion is that the breathing rate is not a good indication of the reception of sound stimuli. A stimulus which did affect the breathing rate was that caused by striking the table on which the aquarium rested. Kuroda quite rightly states that this probably involved "jerking movements of the water" and could not, therefore, be considered a pure sound stimulus.

This imposing array of evidence would lead us to suppose that, except perhaps for the catfish, fishes are deaf. This supposition is contradicted by an equally imposing mass of evidence.

Hunter (15) observed that a gun fired near a pool, but behind shrubbery and thus concealed, sent a number of fishes scurrying into the mud at the bottom. Bateson (2) noted various responses to loud noises and perhaps other vibratory accompaniments of blasting, knocks on the wall of the aquarium, and so on. Zenneck (49) reported response in three species of fresh water fishes to a bell vibrated under water. Several killifishes were observed by Parker (34) (35) to respond by movements of the pectoral fins to the vibration of a bass viol string attached to the wooden end of the aquarium and to the vibration of an electrically driven tuning fork in contact with the aquarium. The frequency of the fork was 128 vibrations per second. Bigelow (4) observed responses made by goldfishes to a fork (100 vibrations) similarly mounted. Four of the six species tested by Lafite-Dupont (24) responded to taps on the aquarium.

Parker presents evidence that the squeteague, Cynoscion, (37) and the dogfish, Mustelus, (38) are sensitive to various sounds. Parker and Van Heusen (39), who used eight species, but most frequently the catfish, report responses to all of the following types of disturbance: a shrill whistle; drops of water falling 5 centimeters to the surface of the water; a blow from a lead ball weighing 335 grams, suspended from a 118 centimeter wire and swinging through an arc of 5 degrees so as to hit the 3 centimeter thick slate end of the aquarium with a speed of 30 centimeters per second; the vibration of a telephone receiver contained in a submerged toy balloon (the frequencies were 43, 86, 172, 344 and 688).

Manning (27), using a telephone receiver as had Parker and Van Heusen, observed response by the goldfish to all the frequencies just mentioned and also to the higher rates of 1376 and 2752 vibrations per second. Finally, Schiche (45) reports the twitching of the back and tail fins of the catfish in response to tapping on the side of the aquarium. This response gradually disappeared when the stimulus was frequently repeated.

How are we to reconcile these contradictory results?

Unfortunately, in few cases has the published report contained an adequate

definition of the experimental or observational conditions. The discrepancies can not be accounted for solely on the bases of the species of fish used because contradictory results have been obtained with the same species. (But, in general, the catfish has most consistently yielded positive results.) Nor does the locale of the work explain the discrepancies, since we find each side represented by both laboratory and field observations. The type of stimulation used serves little better as an explanation, since responses have been claimed and denied for practically all ranges of frequency, intensity and timbre of sounds produced in the air and beneath the water. Neither these nor other conceivable variables, such as the age of the subjects, seem entirely adequate as a basis for the reconciliation of these and the many other similar reports.

No doubt the single factor which, more than any other, explains the conflicting conclusions relates to the methodology employed by all these investigators. The stimulus-test method, as generally used, necessitates the interpretation of behavior by the observer. Even though all of these investigators had been present at all of the investigations agreement might not have resulted. Each would have interpreted the behavior of the fishes in his own way. The fundamental data of the method consist in the interpretations of such general and various movements as those of the fins, tail, gills and eyes (movements the actual stimuli of which are assumed but not controlled). Uniform conclusions can scarcely be expected. In some of the experiments but a single test was given an individual fish. In practically no instance have secondary cues been rigidly controlled.

While space can not be given to the criticism of each report, something may be said of the nature of these uncontrolled stimuli. In some cases these may have been visual, as when strings were vibrated or hands were clapped over the water. On occasions the fishes may have been reacting to mechanical stimulation other than sound. Thus when a tuning fork was brought into contact with the aquarium, the making of the contact, not the vibration thereafter, may have been the effective stimulus. If so, it is quite likely that mechanical disturbance of a frequency much lower than sound may have aroused the fish. This comment applies, also, to the several tests in which the aquarium was tapped.

It should be noted that frequently the stimulus used has not been so accurately defined that it could be reproduced exactly by another investigator. This seems especially regrettable since sound stimulation is so much more readily controlled and defined as to form, intensity and duration than are many other kinds of stimuli, olfactory, for example.

A physiological method

Fortunately, in formulating our opinion regarding hearing in fishes we are not limited to the results of experiments using the stimulus-test method. There is evidence from two other sources, although the number of individual investigations that can be mentioned is small. Piper (41) (42) succeeded in measuring the action-currents in the eighth cranial nerve of the pike and the eel, correlating their character and duration with sound stimuli. The dependence of the currents upon such stimulation was demonstrated. This does not prove that these fishes are sensitive to sound, as that phrase is usually understood, for no functioning of the motor side of the arc was demonstrated.

Nevertheless, these experiments indicate that the animals possessed adequate receptors for sound.

The learning method

Further evidence is found in those experiments involving the behavior modification of the animal on the basis of sound stimulation. An informal, and perhaps accidental use of this method is reported by Krausse (20). He studied the behavior of a single dwarf sheatfish, using various noises and tones. Unlike many other observers, he was not satisfied with one, two or a half dozen observations. He studied the animal intensively over a period of several weeks. As a result, he noticed changes in the behavior of the animal toward sounds. During the early part of this period almost any sound would send it scurrying for shelter. Later, certain sounds which usually preceded the feeding period would bring the animal to the surface. While this work almost entirely lacks control of the stimulus, that of Westerfield (48) is rather more conclusive. This investigator studied the behavior of eight mudminnows (Umbra) kept each in a separate battery jar. For six weeks these fishes were fed snail-meat daily by means of forceps held just above the surface. Before and during the feeding, low D (288 vibrations) was sounded on a ukelele, the base of which was held firmly against the jar. By the end of this time all had learned to jump out of the water a centimeter to get the food. Then began a training period during which each day there was offered a bit of meat and also a bit of grey paper soaked in spirits of camphor. The low D accompanied the offering of the meat as before. The note, A (426 vibrations) was sounded before and during the offering of the obnoxious paper. The order in which the two objects were offered was altered irregularly. Although at first the fishes rose to either bait, in the course of another six weeks they learned to rise only when the low D was sounded. It seems highly probable that had the fishes been responding to mechanical stimuli other than sound they would have taken the bait for either note. There remained the possibility that they were able to distinguish, visually or otherwise, the snail meat from the paper. In a control series, the "meat" sound accompanied the paper, and *vice versa*. During this series, which lasted for three successive days, each of the eight fishes rose when presented with the paper and the low D. Not once did they rise for the meat.

McDonald (31) tested the ability of six blunt-nosed minnows, *Pinephales notatus* Rafinesque, to form an association with the sound produced by a bass viol string (96 vibrations) stretched on the aquarium. Before training, their response to this stimulus was a negative one, that of darting to the bottom of the tank. After being fed regularly when the note was sounded they learned to swim up to the point where the food was usually presented in anticipation of the reward. Secondary cues of a visual nature appear to have been controlled. The fishes were tested as a group and no quantitative data on the formation of the association are presented.

In a recent article Froloff (10) describes behavior modification in "many" fishes of four fresh water and six marine species. Two sounds were used in the study. One was produced by a tuning fork and transmitted to the water by means of a submerged telephone receiver. The other was produced by an electric bell above the water's surface. No fish made any observable response to either. Had he been using the stimulus-test method he

would have stopped at this point and concluded that his fishes could not hear the sounds. But he attempted to train the fishes on the basis of the sounds and succeeded. Electrodes were attached to the fishes (they were tested individually). When given a light electric shock a fish would flee to the bottom of the tank. This movement was conveyed to a kymograph by the same wire which transmitted the shock. During a training series, a sound was presented with the shock. After this training the fish would flee each time the sound stimulus was given and even though it was *not* accompanied by the electric shock. Since secondary cues seem to have been controlled the sound must have formed an effective part of the animal's environment. It might be objected that the use of electric shock may have rendered the fishes too abnormal to be considered typical of their kind. Froloff arranged the sounds used on the bases of the ease with which associations involving each could be established: 1) Sounds produced beneath the water; 2) Sounds produced immediately above the surface; 3) Sounds produced at a greater distance above the surface.

Bull (6) followed the general method used by Froloff, although he made many modifications in detail. Vibrations transmitted to the aquarium by an electric buzzer had no apparent effect upon an eel, *Anguilla vulgarius*. But after the eel had undergone a training series in which the sound was accompanied by an electric shock the animal gave a flight response to the sound alone. A similar experiment, but with blennies, *Blennius gattorugine*, for subjects, yielded negative results. Prolonged training did not alter the total indifference of these fishes to a variety of sound stimuli. In the same article Bull reports an experiment with *Crenilabrus melops* L. in which the food-seeking response was associated with a sound stimulus. In this case he used an electrically driven tuning fork (128 vibrations), the prong of which was held firmly against the outer surface of the tank.

Thus many, though not all of the fishes tested by the learning method appear capable of associating one or another response with a sound stimulus. We feel reasonably safe in denying the often heard statement that all fishes are deaf. Since so many experimenters report negative results it seems probable that, as a group, fishes are not so inclined to make overt responses to sounds as are birds and mammals. Probably, too, the minimum intensity of an effective sound stimulus is higher than for most air-living animals. Sounds transmitted through the air to the water must be of great intensity to be effective. The range of effective frequencies no doubt varies widely from one species to another. There is certainly wide variation in sensitivity, and there are probably some species which are practically insensitive to sound. These specific differences are in accord with the decided anatomical differences in the receptors.

THE RECEPTORS

Cutaneous stimuli

The structural basis of sensitivity to the several kinds of mechanical disturbance merits our consideration. The entire integument of fishes is supplied with simple nerve-endings for the reception of cutaneous stimulation. It seems that certain regions are more plentifully supplied than others. The snout, the anal region and the barbels (when present) are peculiarly sensitive. There are organs peculiar to certain fishes and, it is said, quite unlike those found in other animals, which may be involved in re-

ception of mechanical stimuli. Such structures lining the edge of the scales of the male killifish have been termed contact organs by Newman (33). These are distributed on those parts which during mating come into contact with the female. It is said that the exact distribution varies in the four species studied and that this variation conforms with the breeding habits of these animals.

Hydrostatic pressure

Fishes are subjected to a special form of mechanical stimulation as a result of their environment. The weight of the water is so great that a slight change in depth is accompanied by a decided change in pressure. No study appears to have been made of the sensitivity to these changes nor of the receptors stimulated by them. It has been conjectured that these are located in the air-bladder, an organ possessed by most fishes.

Water currents

Our knowledge of the end organs particularly sensitive to water currents is not complete. Undoubtedly currents when directed at a specific point on the outer surface of the animal with sufficient concentration and force produce a deformation of the integument affecting the cutaneous nerve-endings. But when speaking of stimulation by water currents most writers refer to the effect produced by the flowing of large masses of water past the fish. This effect is presumably that of friction rather than deformation. It has been supposed on rather vague grounds that extreme sensitivity to stimulation of this sort might help account for the direction of migratory movements of fishes. Schulze (46) hazards the suggestion that the lateral line organs are the receptors for such stimuli. Hofer (14) is of the same opinion. The position of these organs, extending along the greater length of each side of the fish from head to tail, may have suggested this supposition.

Parker opposes this view. Using the killifish, he found that response to water currents was not destroyed even though the lateral line nerves were severed (34). Further work (35) of a similar nature led him to the conclusion that "surface waves and current action . . . . must stimulate the general cutaneous nerves." Only indirect evidence is supplied, however, for his attempt to prove the point by elimination of the cutaneous nerves with cocaine failed at this time. More recently, Parker and Van Heusen (39) claim to have successfully eliminated the functioning of the skin as a receptor for a period of an hour and a half by a five minute application of a 20 per cent solution of magnesium sulphate. Under these conditions the animal (a catfish) failed to respond to water currents which always aroused normal catfishes. The conclusion is that it is chiefly the free endings of cutaneous nerves which are stimulated by the gentle flow of water currents.

Low frequency vibration

There has been disagreement as to what organs are receptors for low frequency vibration. In 1904, Parker (36) undertook the determination of the function of the lateral line organs and satisfied himself that they were not involved in the reception of the following types of stimuli: light, heat, salinity of water, food, oxygen dissolved in the water, carbon dioxide, foulness of water, hydrostatic pressure, steady currents of water, and sound. The only stimulus which seemed to affect them was water vibration at the rate of about 6 per second. When the nerves which supply the lateral line organs were cut, a fish failed to respond to such

stimulation, although a normal fish reacted to it decidedly. Hofer corroborates Parker on most points. His criticism of Parker's operative technique may be justified, however. He maintains (14) that the operations performed by Parker deinnervated the cutaneous end-organs of the head region. Hofer believes these to be sensitive to low frequency vibration. He reports sensitivity to stimulation of this general type in several fishes (*Cyprinus carpio*, *Cottus gobio*, *Esox lucius*) in which lateral line innervation had been destroyed. Unfortunately, his method of stimulus presentation may have afforded the fishes secondary cues.

In later work Parker, too, notes that the skin plays a part. Working with Van Heusen (39), he observed the effect of water disturbances at the rate of 4 to 5 per second upon normal catfishes and upon those which had been partially incapacitated in various ways. The aquarium was placed upon an inflated bicycle tire and rocked, apparently by hand, upon this support. Fishes near the surface of the water during this disturbance responded practically normally when either the lateral line organs or the ears, or the two simultaneously, were rendered functionless. When, however, skin reception was eliminated, the sensitivity was somewhat reduced. This seems to point to the integument as the chief source of reception. When the fishes were in mid-water during the disturbance the ear seems to have played almost as important a part in the reception as did the skin.

In a more recent investigation, Rode (44) used a mechanical method of producing low frequency vibration. The rather meager description states that an electrically vibrated blade was employed. The frequency could be varied. We suppose that the blade was of metal and vibrated beneath the water. A number of species was used, including both scaled and non-scaled fishes. Rode reports that the latter were sensitive to vibrations having a frequency of 4 or 5 per second. (The catfish, which was used by Parker, is non-scaled.) On the basis of operative elimination of function Rode came to the conclusion that the lateral line was a receptor for low frequency vibration, but only in conjunction with the ear.

Conclusions on this topic hardly seem warranted at present.

Sound

Naturally enough, only investigators who believe that fishes respond to sound have been interested in the structural basis of hearing. In their opinion, the end-organs responsible lie in the ear. Fishes possess only membranous labyrinths, there being no structures analogous to the middle or external ear of higher vertebrates. The ear, then, consists only of a vestibule and three semicircular canals (except in Cyclostomata, which possess but two). It is the vestibule which contains the supposed sound receptors. Lying within this structure, suspended in the fluid (endolymph) are calcareous otoliths, varying widely in size and number from one species to another. In general, they are numerous but minute in the more primitive fishes, fewer and larger in the more specialized forms. It is supposed that nerve-endings in the walls of the otocysts are involved in sound reception. In certain fishes, such as the carp and the catfish, there is a system of bones communicating with the air-bladder. This organ may receive vibrations which are then transmitted to the ear, in a way analogous to the transmission of vibrations in higher vertebrates from the tympanum to the inner ear by means of the bones of the middle ear.

Parker (34) observed the responses of the killifish to the vibration of a bass viol string, one end of which was fastened to the wooden side of the aquarium. Normal fishes responded about 9 times out of 10. So did fishes whose lateral line and cutaneous organs (except in the region of the pectoral fin) had been deinnervated. Those whose two eighth nerves had been cut responded but once out of every five times. When a tuning fork (128 vibrations) was used as a stimulus, similar results were obtained. The indication is that the ear is the chief receptor. This view has received support in Bigelow's work on the goldfish (4). The method was much the same. An electrically driven tuning fork (100 vibrations) was the stimulus. Control against the effects of operative shock was attempted by sectioning but one of the eighth nerves in a fish. An animal so treated responded as did a normal fish.

Parker (37) was able to make a more exact determination of the part of the ear involved because of the peculiar construction of this organ in the squeteague, Cynoscion. The utriculus and sacculus in this fish are relatively separate structures, not being joined by a canal. It was possible to destroy the utriculus and the semicircular canals, still leaving intact the sacculus and its contained otoliths. This operation did not interfere with normal response. Although it was impossible to make a separate destruction of the sacculus, Parker found that he could press a pin through this organ in such a way as firmly to fix the largest otolith, the sagitta. This procedure appeared to reduce the sensitivity of the animal. The stimulus used must be criticized as offering secondary cues. It consisted of tapping against the wall of the tank. Although we cannot, therefore, define exactly the stimulus to which the animal was responding, it may have been sound. If so, the conclusion would be that the sacculus is the chief organ of this fish involved in hearing.

Certain experiments indicate that two tones which differ considerably in frequency may affect different end-organs. In tests made by Parker and Van Heusen (39) the vibrations produced by an alternating current generator were transmitted to a telephone receiver which was contained in a submerged toy balloon. Frequencies of 43, 86, 172, 344, 688, 1376, 2752 vibrations per second were used. Normal fishes responded to all but the two highest frequencies. Fishes with functionless ears failed to respond to rates of 344 and over but did respond to those below that. It appears that the integument must have mediated in the reception of the lower rates.

Manning (27) has recently studied the goldfish, using the sound production device which has just been described. The normal fish seemed to respond to the entire range of frequencies. Removal of the sacculus-lagena destroyed the response to the two highest rates. Removal of the utriculus destroyed the response to 344 and 688 vibrations. When both of these parts were destroyed the fish responded to only the two lowest rates. In this respect such a fish resembled one in which the entire ear had been removed. Manning concludes that the "large disc-like otolith of the lagena is the receptor for high rates, the utriculus for medium and low rates, the skin and lateral line organs are additional receptors for low rates up to and including 344." No data are given.

SUMMARY

A brief summary of this material must be considered as very tentative. Our knowledge of fishes is based on the study

of but an infinitesimal fraction of the species that are known to exist. And among the few species with which we are somewhat familiar we do not find uniformity with respect to sensitivity to mechanical stimulation. The following generalizations have, therefore, a restricted value.

Tactual stimulation

The sensitivity to light touch seems to be well developed. Most fishes strenuously avoid even gentle contact with objects; a few seek such contact. Discrimination between gentle stroking and scratching has been demonstrated. The cutaneous nerve-endings are responsible for the reception. In some cases there are also special contact organs.

Some observations suggest that fishes are not particularly responsive to more intense tactual stimulation, such as prodding.

Water currents

Currents directed at a portion of the integument arouse a prompt response due, probably, to stimulation of the cutaneous nerve-endings. To a large flow of water some fishes usually orient positively, some negatively. It has been conjectured that the lateral line organs act as receptors. There is discrimination between currents flowing at different rates but how acute this is, is not known.

Hydrostatic pressure

There seems to be no experimental work on this subject. It has been conjectured that nerve endings in the air-bladder may mediate in the reception of this stimulation.

Low frequency vibration

All fishes seem decidedly sensitive to water disturbance of this kind. It has been thought that blinded fishes in avoiding objects depend upon a low frequency "echo" of the water disturbance caused by their own movement. If so, their acuity must be remarkable, for they can avoid obstacles even when the water in which they swim is in a condition of general disturbance. Probably the integumental nerve-endings, rather than the ear or lateral line organs, are responsible for this sensitivity.

Sound

There are wide specific differences in sensitivity to sound. Some species may be deaf. There are also differences in the range of effective frequencies. Some fishes can apparently discriminate tones differing considerably in pitch. But generally speaking, the hearing of fishes is certainly inferior to that of air-living vertebrates. Sounds produced under water are more effective stimuli than those produced in the air above the water. Fishes appear to be far more sensitive to the lower frequencies, and most sensitive to frequencies below those falling under the term sound. It is probable that the ear is the receptor for tones of several hundred vibrations a second and more. Frequencies below this affect the integumental nerve-endings and, possibly, the lateral line organs.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

GOD MEANS ANYTHING TODAY

Being a review of *Has Science Discovered God? A Symposium of Modern Scientific Opinion.* Gathered and Edited by Edward H. Cotton. New York (Thomas Y. Crowell Co.), 1931. 5¾ x 8⅝; lviii + 308. $3.50.

By Francis P. Le Buffe, S. J.

This book is a symposium made up of selections from the writings of sixteen different scientists, all of them of outstanding attainments. Its purpose is praiseworthy: to show that through modern science "we are closer to the First Cause, and the purpose back of creation, than we ever have been." (p. 298.) The compiler has gathered many various and even contradictory opinions, but, despite this contradictory variety, he judges his contention to be true.

Heber D. Curtis states succinctly just why the question which provides the title of the book is asked (p. 53):

"Science is today taking stock of the beliefs it has permitted itself to make; in a closer alliance with that field formerly preempted by philosophy it has subjected its basal principles to close analysis. Somewhat to his surprise, the modern scientist finds that he has gradually and without prevision reached a point where he is permitting himself, even demanding, beliefs and theories which involve the transcendental and the infinite."

Because I am a theologian I have been asked to review this book; and precisely because I am a theologian I find the task a difficult one. And the reason for this difficulty is manifold.

It lies first in the underlying, and everywhere apparent, supposition that science—in the sense of physical science—*can* prove the existence of God. The very title of the book jolts the philosopher and the theologian who necessarily philosophizes. Physical science as physical science is as powerless to discover God, as a test tube is to discover gratitude. This is acknowledged by some of the contributors and notably by that ever-fascinating writer, Sir J. Arthur Thomson:

"Science aims at an analytic and genetic description of the world and of all that goes on there, in so far as scientific methods can be applied. It is not the only kind of knowledge; it is partial and abstract, for it deliberately restricts itself to verifiable factors that can be measured or registered, observed again and again, and even experimented with." (p. 165.)

"Science may be defined as a criticised and systematised body of verifiable knowledge, based on observation and experiment, and summed up in the lowest common denominators available at the time." (p. 167.)

And with equal clarity, another delightful writer, Arthur S. Eddington, states (p. 47):

"Starting from aether, electrons and other physical machinery we cannot reach conscious man and render count of what is apprehended in his consciousness. Conceivably we might reach a human machine interacting by reflexes with its environment; but we cannot reach rational man morally responsible to pursue the truth as to aether and electrons or to religion."

And this brings us quite immediately to the viewpoint that is had in common by a number of the contributors: that if God is to be found, He *must* be found by the mechanics of physical science. The first and ready reply is that if He were found there, He would be merely matter, and therefore not God in any acceptable meaning of the word. And the second reply is again given by Thomson (p. 168–9):

> To be guarded against is a too ready acquiescence with the widespread view which regards science as a bedrock kind of knowledge, deeper and surer than all other attempts to understand and appreciate . . . . Therefore when the matter-of-fact man inquires: "What has Science to say about it?", he is asking a necessary question—would that it were oftener put, but he is deceiving himself if he thinks that the scientific answer is the last word on the subject . . . .
>
> Science aims at a *description* of things and processes in terms of the Lowest Common Denominators available, while Theology and the intellectual activities of Religion aim at some transcendental *interpretation*, in which the highest concept is that of a Supreme Reality—God. Between the scientific description and the religious interpretation there should be no antithesis, for they belong to different universes of discourse, yet they must be consistent, for we cannot tolerate idea-tight compartments in our minds.

With his usual felicitous clarity of expression, Thomson here assails two different camps: on the one hand, the Positivists who wish to weigh and measure everything or else declare it non-existent; and, on the other hand, both the theological Modernist who dissociates his idea of God from all physical data, and the Fundamentalist who flies in the face of scientific data and proclaims the world to be flat or the original creation to have taken place in six twenty-four-hour days.

A third major difficulty is the rather hopeless confusion as to the meaning of a *personal* God. Not only did the present reviewer read this book, but he wrote to each of the contributors, asking this one question: Do you believe in a personal God, distinct from the universe, to whom man has definite obligations? (The frankly atheistic Huxley succinctly states (p. 105) that the question is that of "an independent or unitary being, active in the affairs of the universe.") The answers received to the letter betrayed even more clearly than the excerpts in the book—for the book is after all only a symposium and that, too, largely of excerpts from books heretofore printed—that this one concept of the *personality* of God needs clarification prior to any discussion that will prove worth the time and the energy therein spent. Many scientists balk at "personality" in God, because they rightly balk at "bodily-ness" in God.

Now in the language of philosophy and theology a *person* is an *existent having intellect and will*, an *existent having mind*. Whether this existent has or has not a material body (such as our human body) does not enter, in any least wise, into the definition. The whole discussion centers on "intellect and will." The *intellect* is the power to have non-dimensional concepts such as "three-sidedness," "loyalty," "patriotism," "humanity," "duty," "right;" and the *will* is the power to act or not to act, to do or refrain from doing that which the intellect has conceived.

The question, then, of God's existence, is clearly this: Does or does not the universe demand an intelligent Being independent therefrom and, at least, initially directive thereof as to its physical laws? If so, then man, gifted with a knowledge of such a Being, must acknowledge Him, and further acknowledge certain duties towards Him.

For this is the essential definition of religion: the acknowledgment of a set of truths concerning God and of a consequent set of duties toward God. That is religion in its stark essentials. Religion is, therefore, in its ultimate concept totally independent of "ecclesiasticism," of "Church," of "community services." If there were only one man alive, religion would find place. (We are leaving aside, of course, any question of a revelation from God and taking simply the concept of religion as it can be known by the unaided human mind.)

Religion, therefore, is primarily a matter of intellect and will: the knowledge of the existence of a personal God and of the consequent duties, and the will to live up to and abide thereby. Close adherence to this essential concept protects against two false notions of religion.

The first error lies in estimating belief in a personal God to be a blind leap in the dark, a mere unreasoning adherence to Something that must be true, though we lack all proof that it is true. This error is indicated by what John Langdon-Davies writes (p. 213):

"To what is known he adds an over-belief, something which cannot be proved, but which, on the other hand, cannot be disproved by the body of natural knowledge on which it is built. This over-belief is a man's religion."

Huxley, atheist that he is, answers correctly (p. 106): "Knowledge is inevitably the most important raw material of theology." Indeed, if there be not *real knowledge*, belief in God is merely a "hunch" and God is merely the object self-created by the believer to satisfy his wants, like unto the dream of food which hunger stirs up in the starving.

Another error (and this leads to the loss of faith in God, or, more accurately, in some cases to the loss of any sense of obligation to attend church service) is due to a revolt on the part of an intellectual man against mere emotional religion. Conklin puts this well (p. 87):

"The real causes of loss of religious faith on the part of many scientists and scholars are to be found primarily in a revolt against ecclesiasticism, literalism, formalism; and against an insistence upon the faith of childhood as contrasted with the results of science."

Emotions do, indeed, find their place in religion, just as they do find their place in a very large per cent of all human activity. But true religion is founded on reason. Of course, thereafter and therefrom flow emotions. (Just as *rational* love between man and woman is based on the appreciation by the mind of their general fitness each to each, with emotions inevitably flowing therefrom. "Calf love" is all emotion and is not worthy of the name of human love.)

In view of the above, it is not the function of religion merely "to paint the picture of the world that ought to be, and to inspire men with zeal adequate for its realization" (Mather, p. 17), nor to secure "the development in the individual of a sense of social responsibility" (Millikan, p. 27). These functions are entirely secondary to the main function of religion: to recognize the existence of God, to recognize His will in our regards, and to follow that will. Part of His good pleasure in our regard certainly is to have us work for the realization "of the world that ought to be" and "the development . . . of a sense of social responsibility." These are very real ends, but, to one who believes in a future life, they are partial and immediate ends, leading, *through their fulfillment*, to further, higher and ultimate ends.

The question—"Does God exist?"—is, then, in its simplest form: Is there an Existent possessed of intellect and will to whom the origin of the universe, man included, is due and to whom consequently man has duties?

The reply comes: If there is order in the universe, if there is purpose shown therein, then there must be Someone who put the order there, Someone who conceived the purpose. To identify this Someone with the universe itself, as Einstein and others do, is to empty religion of all meaning, as Huxley again rightly points out (p. 105):

"To the religious philosophers who conclude that the universe is God, and to those others who identify God with the philosophical Absolute, the Unknowable behind phenomena, the unifying principle in reality, and so forth, we may legitimately reply that their conclusions may be of great interest for philosophy, but have ceased to have any but the remotest bearing on religion. Such a God could not be worshipped or prayed to, and could not arouse the intense emotion or ecstasy of mystical experience, and, in fact, has really no kinship with the actual gods of actual religions."

Proofs, then, for the existence of God—a Being with intellect and will, and therefore a person—are drawn from various fonts: purpose, order, etc., and, indeed, from the mere *existence* of things about us. Indeed, even though there were no order in the world, even though there were no purpose, the bare fact of one single existent—formerly not existing and now existing—would demand a Creator. Re-

gress to an infinite series of such "contingent" beings or "beings-that-happen-to-exist" does not solve the matter. If link 1 hangs from link 2, and 2 from 3, and so on, I do not solve the problem of support by asserting that the chain is infinite in length. I increase the problem: an infinitely long hanging chain requires an infinitely strong peg to hang from. Sir James Jeans, one of the contributors, has taken definite stand against such an infinite series. He writes thus (pp. 248–9):

"Modern scientific theory compels us to think of the creator as working outside time and space, which are parts of his creation, just as the artist is outside his canvas."

Elsewhere he writes:

"And so [the universe] must have had a beginning; there must have been what we may describe as a 'creation' at a time not infinitely remote." ("The Mysterious Universe," p. 154.)

"And wherever we fix it [the beginning], our next step back in time leads us to contemplate a definite event, or a series of events, or continuous process, of creation of matter at some time not infinitely remote. In some way matter which had not previously existed, came, or was brought into being." ("The Universe Around Us," p. 316.)

"Traveling as far back in time as we can, brings us not to the creation of the picture, but its edge; the creation of the picture lies as much outside the picture as the artist is outside his canvas." (Ibid., p. 317.)

That there is purpose in the world is quite increasingly admitted by scientists today. After a few decades of stark materialism they have come back to the position of their older predecessors: that purpose is written out large across the book of Nature. Mather says (pp. 9–10):

"There is uniformity of purpose, rather than uniformity of action. . . . Things were done in order that certain objectives may be reached in the future."

And Conklin (p. 88):

"It seems to me much more probable that matter, energy, life and mind,—that the principles, laws and, in general, the order of nature are evidences of the immanence of some plan in this universal mechanism which we call 'nature'." . . .

And George Thomas White Patrick (pp. 137–8):

"Science does not yet understand the causes of progressive organization, but it certainly presents no aspect of a chance affair. Some creative agency seems to be at work."

Even where purpose is denied, one finds a tacit and implied admission. Huxley (p. 110) says science gives us the picture of "a universe of appalling vastness, appalling age, and appalling meaninglessness." Yet, despite this statement he cannot help falling into admission of purpose when he says (p. 113): "Our minds, in fact, like our bodies, are devices for helping us to get along somehow in the struggle for existence." If they are "*devices for helping*," then there is purpose back of them. The very word "device" implies purpose.

A misleading note, echoed many times elsewhere, is struck by Kirtley F. Mather (p. 6):

"Only that religion, which is in harmony with the current scientific description of man and the universe, can maintain itself effectively in any age."

Is not that asking too much? Or rather has not Dr. Mather slipped up on his words? "*Current scientific description*" is notoriously a shifting thing—just to mention the present-day conflicting attempts of Jeans, Lemaître, Einstein, etc., etc., to describe the universe; or, within the last twenty-five years, the continual assertion, denial, counter-assertion, revision, on the single question of man's non-human ancestry. It cannot be insisted on too much that scientific *facts*, and scientific *interpretations* of facts or description of *supposed-facts* are different things, poles-apart. As Curtis says (p. 59):

"As we look back over the discarded scientific theories of the past, once regarded as inspired and now only of historical interest, we are more and more forced to the conclusion that in the final analysis any scientific theory is simply a belief."

Again Eddington says (p. 49):

"The lack of finality of scientific theo-

ries would be a very serious limitation of our argument, if we had staked much on their permanence. The religious reader may well be content that I have not offered him a God revealed by the quantum theory, and thereafter liable to be swept away in the next scientific revolution."

Conklin is right (p 89):

"Science cannot solve the great mysteries of our existence,—why we are here, whither we are bound, what it all means. Faith alone assures us that there is definite purpose in all experience. This knowledge makes life worth living and service a privilege."

But belief in the existence of God *is* knowledge: sound reason based on fact. For there is, indeed, a sense in which *science* has discovered God. But this discovery is ages old, for *science*, in this sense, predates all measuring-, weighing-, scrutinizing-implements. Dr. Curtis (p. 57) says:

"I personally prefer another definition for science. Science is the body of things known, with the attempt to arrange *all* things in an *orderly* fashion. The important words are italicized with intent. The first one indicates that science need place *no limits whatever* on its attempts to explain things. Under this ruling, the speculations of religionists and philosophers regarding a First Cause are a form of science."

To the Scholastic philosopher this sounds very familiar for, until Positivist philosophy denatured the word, *science* meant precisely "systematized knowledge of many things that come under a common category." Hence the science of light, of sound, of heat; the science of mind; the science of philology; the science about God—which is theology.

It is this science about God—Theology —which has given us clear and definite ideas of God. From no one conversant with such Theology will the admission come that, through modern scientific data, God has become "a far more convincing, majestic reality." (Cotton, Introduction, p. lv.) No, at least not to the Catholic thinker, nor, we believe, to any one who had an idea of God which was at all correct. For God, traditionally is the "creator," "the maker from nothing." Now the jump from "nothingness" to "something-ness" is the big jump; it is *the* jump that requires infinite power. Whether the jump be from nothingness to a universe $5' \times 4' \times 3'$ or to a universe billions of light years in diameter adds no jot or tittle to the infinity already required.

As the theologian closes the book, he recalls the words of Eddington ("The Nature of the Physical World," pp. 348–9):

"A besetting temptation of the scientific apologist for religion is to take some of its current expressions and after clearing away crudities of thought (which must necessarily be associated with anything adapted to the everyday needs of humanity) to water down the meaning until little is left that could possibly be in opposition to science or to anything else."

It would almost seem that Eddington had in mind the words of Durant Drake ("Seekers After God," *Harvard Theological Review*, January, 1919):

"If there is less avowed atheism today (and certainly atheism is less blatant, if perhaps more widespread), it is because it seems now rather needless to be an atheist. There are so many conceptions of God afloat that anyone at all widely read can scarcely fail to find one suited to his mental outlook and convictions."

To make a word meaning anything one pleases is to nullify all discussion. Yet it is patently clear to anyone who reads the present volume, that the word *God* means anything today. Traditionally it means only one thing, and that one meaning is the only meaning worth bothering about. It means: a Being independent of the universe, gifted with intellect and will, towards whom man stands in a definite relation of subservience. Has physical science discovered this God? No—nor will it ever. But has right reason, which is also truly *science*, discovered this God? It has.

BRIEF NOTICES

EVOLUTION

EFFECTIVENESS IN NATURE OF THE SO-CALLED PROTECTIVE ADAPTATIONS IN THE ANIMAL KINGDOM, CHIEFLY AS ILLUSTRATED BY THE FOOD HABITS OF NEARCTIC BIRDS. *Smithsonian Miscellaneous Collections, Volume 85, Number 7.*

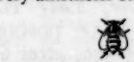
By W. L. McAtee. Smithsonian Institution, Washington. 75 cents. 6¼ x 9½; 201; 1932; paper.

The author's thesis is that the so-called protective adaptations of animals are not really effective; that they do not protect; and that consequently natural selection cannot account for their existence. The evidence adduced consists primarily of the stomach contents of about 80,000 birds, examined in the United States Biological Survey since 1885. In this material a total of 237,399 different species have been identified. By comparing the proportions of species of different phyla in this material with the estimated total number of different species living, it is found that the different phyla are eaten in rough proportion to their numbers; and further, that within the class of insects the same holds for the different orders and families.

Availability undoubtedly is the chief factor involved in the choice of food, and predation therefore tends to be in proportion to population.

Considering bird predation alone this principle leads to a high degree of indiscriminancy in attack upon the whole kingdom of animal life. The combined attack of birds plus all other predators still more closely approaches complete indiscriminancy. In other words there is utilization of animals of practically every kind for food approximately in proportion to their numbers. This means that predation takes place much the same as if there were no such thing as protective adaptations. And this is only another way of saying that the phenomena classed by theorists as protective adaptations have little or no effectiveness.

Natural Selection theories assume discrimination in the choice of prey. The principle of proportional predation so obvious from the data contained in this paper vitiates those theories for it denotes indiscrimination, the very antithesis of selection.



SOCIETAL EVOLUTION. *A Study of the Evolutionary Basis of the Science of Society. Revised Edition.*

By Albert G. Keller. The Macmillan Co., New York. $2.50. 5 x 7¼; ix + 419; 1931.

An exploration of the nature of social variation, social selection, social transmission and social adaptation. The idea was suggested by the Darwinian system, and the analogy is obvious.

But the contention here is that behind the analogy lies a broad identity. There is something in the social range which *is* variation (difference), whether or not it may be *like* what is called variation in the organic field; similarly social selection *is* selection (elimination), not merely *like* it. In the social realm, also, there is a means of transmission (securing likeness) having the essential attributes of heredity in nature; and adaptation occurs in one range of phenomena as in the other. . . . If the salient and determinative processes of organic evolution are repeated in their essence in the life of human society, then we can say that there is such a thing as societal evolution and that we have some definite idea of what it is like.

The development of this idea should be useful in securing an orientation for the study of human society.

CATALOGUE OF THE MARINE PLIOCENE AND PLEISTOCENE MOLLUSCA OF CALIFORNIA AND ADJACENT REGIONS. *With Notes on Their Morphology, Classification, and Nomenclature and a Special Treatment of the Pectinidae and the Turridae (Including a Few Miocene and Recent Species). Together with a Summary of the Stratigraphic Relations of the Formations Involved. Memoirs of the San Diego Society of Natural History, Volume I.*

By U. S. Grant, IV, and Hoyt R. Gale. San Diego Society of Natural History, San Diego, Calif. $8.00. 9 x 11½; 1036; 1931.

This volume is divided into two parts. The first is a summary of the stratigraphic relations of the beds in which the fossils that are recorded in the second part were found, based not only upon an interpretation of the evidence offered by the fossils themselves but upon opinions derived from as many other different kinds of evidence as could be brought to bear on

the subject. In the second part an attempt is made to list every species and variety of marine mollusk known to occur in the Pliocene and Pleistocene deposits of California.

LES ANCÊTRES DE L'HOMME.

By Dr. Binet-Sanglé. Albin Michel, Paris. 25 francs. 5½ x 8½; xx + 290; 1931.

The greater part of this book is an attempt to show that man's lineage consists of not less than twenty-nine species ranging from the *Micrococcus*, through the amoeba, fish, reptile, marsupial, anthropoids, etc., as compared to Haeckel's seventeen species, Mathias-Duval's four, and Dohrn and Minot's five. The author attempts to prove this by discussing the problem from the points of view of the embryology, anatomy, pathological anatomy, teratology of man and animals, and paleozoology. He claims to have discovered the following laws: (1) that the duration of the paleozoological stages diminishes from the first to the last; (2) that the duration of the embryological stages increases from the first to the last; and (3) the duration of each embryological stage is therefore inverse to the duration of the corresponding paleozoological stage. Tables and charts are given to illustrate these laws. The book has an index and a bibliography.

DIE STELLUNG DES MENSCHEN IM RAHMEN DER WIRBELTIERE.

By Othenio Abel. Gustav Fischer, Jena. 20 marks (paper); 22 marks (cloth). 6¾ x 10⅛; xvi + 398; 1931.

A critical survey and analysis of the theories advanced concerning the relationship of man to the other primates, based upon a study of fossil remains (mostly skull parts and teeth) and comparisons of the structure of limbs and modes of locomotion of living primates. A chapter is devoted to a critical discussion concerning the location of the cradle of the Hominidae. There are author and subject indices, and literature lists are added to some of the chapters.

THE COOL-WATER TIMMS POINT PLEISTOCENE HORIZON AT SAN PEDRO, CALIFORNIA. *Transactions of the San Diego Society of Natural History*, Vol. VII, No. 4.

By Alex Clark. San Diego Society of Natural History, San Diego, Calif. 6¾ x 10⅛; 18 + map and chart; 1931 (paper).

GENETICS

CONTRIBUTIONS TO THE GENETICS OF CERTAIN CHROMOSOME ANOMALIES IN DROSOPHILA MELANOGASTER. *Carnegie Institution of Washington Publication No. 421.*

By A. H. Sturtevant and T. Dobzhansky. Carnegie Institution of Washington, D. C. $1.75 (paper); $2.25 (cloth). 6⅞ x 10; 81; 1931.

Dr. Sturtevant contributes the first paper in this series on "Known and probable inverted sections of the automosomes of *Drosophila melanogaster*." He describes sixteen new crossover reducers and their interrelations to each other and to the previously known types. The second paper is by Dobzhansky and Sturtevant on "Translocations between the second and third chromosomes of *Drosophila* and their bearing on *Oenothera* problems." The last paper by Sturtevant is on "Two new attached-X lines of *Drosophila melanogaster*, and further data on the behavior of heterozygous attached-X's." Literature lists are given.

MENTAL TESTS AND HEREDITY, *including a Survey of Non-Verbal Tests.*

By Barbara Schieffelin and Gladys C. Schwesinger. Eugenics Research Assoc., Cold Spring Harbor, Long Island. 75 cents (paper) + 15¢ postage; $1.00 (cloth) + 15¢ postage; after 650 copies sold, price $1.50 (paper), $2.00 (cloth). 6 x 9⅛; ix + 298; 1930.

The authors thus describe their aims:

Two things have been attempted in this book: First, the various ways in which mental tests are used in experiments dealing with heredity and environment have been described, and second, a brief, analytical survey of non-verbal tests has been offered. This last might be considered as a first step toward a complete inventory of mental measurements, aiming to locate the best tests for use in eugenic research, which has been defined as "the study of the part played by human affairs in heredity."

The bibliographies will be found useful.

GENERAL BIOLOGY

THE EMERGENCE OF LIFE. *Being a Treatise on Mathematical Philosophy and Symbolic Logic by which a New Theory of Space and Time is Evolved.*

By John B. Burke. Oxford University Press, New York. $7.50. 5½ x 8¾; ix + 396; 1931.

Not a book for the simple minded biologist, we fear. The author discourses on extremely high-toned subjects, largely with the aid of the Boolean algebra of logic, into which he has introduced complex numbers in order to employ the Hegelian dialectic. In his own words:

> The striking feature in Hegelianism is its self-sufficiency. The positivist claims to see only one side and to see that side as clearly as it can be seen to make it positive. The agnostic does not claim to see either side clearly enough to be sure of anything and is overwhelmed by the uncertainty of his insufficiency. But the Hegelian not only sees it partially but sees it whole. If we would but bear in mind the geometrical parallel the difficulty in Hegel might be lightened if not removed. Identity and difference are the same but different aspects of the same yet different things.
>
> In the algebraic treatment of the subject the introduction of imaginary classes involving functions directly or indirectly containing $\sqrt{-1}$, a potential class not actually existing, but admitting on expansion of realization or emergence, enables us to express the remarkable appearance in time, from apparently nowhere, or nothing, of types that had lain hidden as it were, as permanent possibilities of existence. This is the most striking characteristic of the method; and its development entails the very essence of the idea as the all in all and the all containing all. When we consider the expansion of the function that expresses the complete universe of discourse.
>
> $(x_1 + \bar{x}_1).(x_2 + \bar{x}_2).(x_3 + \bar{x}_3)\ldots(x_n + \bar{x}_n) = 1.$
>
> $x_1, x_2, x_3, \ldots . x_n$ being, as usual, any classes whatsoever, then since we could regard each of these as made up of a real and an imaginary element thus $x_n = a_n + ib_n$, where $i^2 = -1$, it follows that as n is increased new combinations of classes will arise, apparently from nowhere. If the element of *time* be taken as a factor, then obviously, if we carefully bear in mind the logical signification of the equation and the actual sense in which it is intended to be interpreted, emergence occurs. For taking $x_n = t^n$, and $t^n = t$, the roots of unity when substituted in the equation of the universe of discourse can be (if the time be regarded, as it should be, in continuous progression) the means of unexpected emergence in the expansion of the factors of time to infinity. If time then be one of the factors, it obviously, sooner or later, must develop all the terms that in the earlier stages lay hidden owing to their association with the imaginary terms or roots of unity; but as time and events unfold, the full evolution of the idea is realized in the emergence of the possibilities as the Divine Idea of Logical Truth. This perhaps is the Hegelian conception of the Idea.

IMMUNITÉ, INTOLÉRANCE, BIOPHYLAXIE. *Doctrine Biologique et Médecine Expérimentale.*

By Arnault Tzanck. Masson et Cie, Paris. 35 francs. 5¾ x 9⅛; 268; 1932 (paper).

The author makes an interesting distinction between intoxication and intolerance as biological phenomena. The essence of the distinction is that the symptoms or lesions of intolerance produced by an infra-toxic dose of some agent, say $HgCl_2$, are always different in kind from the symptoms and lesions produced by a toxic dose of the same agent. For example, if a large toxic dose of $HgCl_2$ is given to each of 100 animals they will all die with practically the same symptoms, in which the renal difficulties dominate the picture. If, on the other hand, a small infra-toxic dose is given to each of another hundred animals, perhaps only ten will show any effects at all, and it will be impossible to predict what symptoms these ten will exhibit—some may have mercurial colic, others erythrodermia only, and so on. The one thing that can be safely predicted is that none will exhibit the anuria of mercurial poisoning. This idea the author develops in many directions. The general biologist and particularly the geneticist will find the book stimulating to thought.

BOLETIN DE LA ACADEMIA NACIONAL DE CIENCIAS. *Tomo XXXI, Entregas 3 y 4.*

Academia Nacional de Ciencias, Cordoba, Argentina. 6¾ x 10½; 250; 1931.

This number of the Bulletin of the Argentine National Academy of Sciences contains three papers of general biological interest, as follows: Sobre el desarrollo de la forma externa de algunos leptodactilidos argentinos, by A. E. Bonjour; Énumeración y difusión de las grandes asociaciones de historia natural, by E. Sparn; Estudios biométricos sobre la variabilidad de la coraza en desdentados con y sin poliembrionia especifica, by E. R. Langmann.

THE MICROSCOPE. *Ultra-Violet Edition (15th). Revised and Rewritten with a Wholly New Chapter on Ultra-Violet Radiation in Microscopy.*

By Simon H. Gage. Comstock Publishing Co., Ithaca, N. Y. $4.00. 6 x 9; viii + 586; 1932.

The material for this new edition of an old and standard treatise has been revised and brought up to date. Of most interest is the description of the new ultra-violet microscope, a device for investigating small animals and tissues under the action of ultra-violet radiation. In the author's opinion organisms should be studied while living or in a fresh and unstained condition, by means of the perfected darkfield, ultraviolet, and polarizing microscopes, as one gets a more accurate knowledge in this way than from stained material. Any laboratory will find this book a useful and valuable reference work.

THE MACHINES WE ARE. *The Principles of Living Phenomena.*

By Robert T. Hance. Thomas Y. Crowell Co., New York. $3.00. 5¾ x 8½; xvi + 382; 1932.

The thirty brief, disconnected essays in this book grew out of a series of radio talks given by the author, and are written with a view to acquainting the general reader with the outstanding facts and prevailing trends in modern biology. Topics embracing the fields of cytology, embryology, heredity, dietetics, physiology, and biological metaphysics are discussed in a light, non-technical vein. A list of suggested reading is included.

FLORIDA WILD LIFE. *Observations on the Flora and Fauna of the State and the Influence of Climate and Environment on Their Development.*

By Charles T. Simpson. The Macmillan Co., New York. $2.50. 5½ x 8½; xii + 199; 1932.

This book, written by a nature lover who has long resided in Florida, is a delightfully informal but trustworthy guide to the many interesting forms of tropical and semitropical plant and animal life to be found in this region. Emphasis is put on the need for conservation of many of these types. The book contains many pleasing illustrations and an index.

KAMONGO.

By Homer W. Smith. The Viking Press, New York. $2.00. 5 x 8⅛; 167; 1932.

This treatise deals with the general biology of two ancient forms—God and *Protopterus*, called Kamongo in Africa. The part about *Protopterus* is the more interesting. The philosophy of the book somehow suggests a subtle and profound amalgamation of the outlooks of Christopher Morley and the late Lyman J. Abbott.

LEBENSRAUM UND LEBENSGEMEINSCHAFT. *Eine Einführung.*

By Friedrich Lenz. Otto Salle, Frankfurt a.M. und Berlin. 4.32 marks 5¼ x 7½; 183; 1931.

A short but good popular account of the general scope, method, and problems of ecology. The bibliography is brief and largely confined to German titles.

GENERAL BIOLOGY LABORATORY MANUAL.

By Leonas L. Burlingame, Arthur C. Giese and Robert E. Hungate. Henry Holt and Co., New York. $1.25. 5½ x 8½; xii + 137; 1931.

This syllabus, prepared for use with Burlingame, Heath, Martin and Peirce's *General Biology*, deals with the following topics: protoplasm, the rôle of green plants, metabolism, adjustment of organisms, association of organisms, rôle of micro-organisms, the community of organisms, growth and reproduction, heredity, evolution and classification, the distribution of organisms, and man. Provision is made both for laboratory experiment and for field observation.

CICATRISATION ET RÉGÉNÉRATION.

By Jacques Millot. Armand Colin, Paris. 10.50 francs (paper); 12 francs (cloth). 4⅜ x 6¾; 204; 1931.

One of a collection of *mises au point*, primarily devoted to the problem of regeneration. A good deal of material is presented in a clear and attractive manner. The illustrations are nothing out of the ordinary. The bibliography is reasonably adequate in view of the scope of the book.

DIE MITOGENETISCHE STRAHLUNG. *Zugleich zweiter Band der "Probleme der Zellteilung."*

By Alexander Gurwitsch, in collaboration with Lydia Gurwitsch. Julius Springer, Berlin. 33 marks (paper); 33.80 marks (cloth). 5½ x 8⅝; ix + 384; 1932.

A useful summary of the more recent work on mitogenetic rays in Gurwitsch's laboratory and elsewhere, much of it previously unpublished. It assumes an acquaintance with the author's earlier work, summarized in his *Problem der Zellteilung*, reviewed here in Volume II. There is a good bibliography.

CHEMICAL EMBRYOLOGY. *In three volumes.*

By Joseph Needham. The Macmillan Co., New York. $35.00. 6¼ x 9⅛; I, xxi + 613; II, xiv + 614–1253; III, xiv + 1254–2021; 1931.

A detailed review of this book will appear in an early number of THE QUARTERLY REVIEW OF BIOLOGY.

HUMAN BIOLOGY

THE MESSIAH JESUS AND JOHN THE BAPTIST *According to Flavius Josephus' Recently Rediscovered 'Capture of Jerusalem' and the Other Jewish and Christian Sources.*

By Robert Eisler. English Edition by Alexander H. Krappe. The Dial Press, New York. $10.00. 5½ x 8¾; xxviii + 638; 1931.

The paucity of references to Jesus in non-Christian sources has generally struck students of the origins of Christianity and has led some of them to conclude that he was a wholly mythical character. In this scholarly book Dr. Eisler gives evidence "that there once existed a rich fund of historical tradition about the Messiah Jesus both among the Jews and the non-Christian Greeks and Romans" and that this precious material was deliberately destroyed by the ecclesiastical censors who since the days of Constantine have suppressed whatever offended against the susceptibilities of the Church. There is, for instance, evidence that the official records of the trial of Jesus before Pilate existed in the Roman archives as late as A.D. 311, when the Emperor Maximin published them as a counterblast to the Christian propaganda. But even an ecclesiastical censor sometimes nods, and passages which were deleted in most manuscripts survived in a few. In particular the Slavonic translations of Josephus, first made known to Western scholars by Berendts, preserved many of the passages which had been deleted in the better known texts. By a comparison of these with quotations from Josephus in Christian polemic and apologetic literature and with the account in the gospels Eisler reconstructs the following narrative:

Jesus was of a branch of the clan of David which had come down in the world

to the status of Rekhabite artisans. In this carpenter and healer, "a man of simple appearance, mature age, small stature, three cubits high, hunchbacked, with a long face, long nose, and meeting eyebrows, so that they who see him might be affrighted, with scanty hair (but) with a parting in the middle of his head, after the manner of the Nazirites, and with an undeveloped beard," the conviction gradually grows that he is the promised liberator-king who is to inaugurate the world-wide reign of peace and righteousness. Gathering about him a small band of disciples, among them his twin-brother and double, Judas Thomas, he sends them out to announce the glad tidings to all nations. When the ambassadors return with the disheartening news that their singular mission has been unsuccessful, Jesus resolves to lead the elect on a new exodus into the desert, away from the Roman bondage. With his disciples he journeys to Jerusalem to make the solemn announcement to the great assembly of pilgrims at the Passover. Amid cries of "Deliver us, son of David " he rides into the city on his donkey. His followers seize the temple, from which the new king drives out the money changers and the dealers in sacrificial animals. His reign, however, lasts only until Pilate with his legions arrives from Caesarea and recaptures the temple. The new king is seized, condemned according to martial law, and crucified along with two other leaders of the revolt. Apparently the movement is crushed. But fleeting glimpses of Thomas, the twin-brother of their Master, start the rumor among the excited disciples that, in fulfillment of his own predictions, Jesus has risen from the dead.

No doubt this version will not satisfy those curious persons who demand that Jesus shall be both historical and mythical, both man and God. It does, however, give a coherent interpretation of many passages in the sources otherwise difficult to explain. We commend the book to all who are interested in Christian origins. For the benefit of those who shrink from reading 600 pages of text Dr. Eisler, in his preface, gives directions for skipping.

MARRIAGE AND DIVORCE 1930. *Statistics of Marriages, Divorces, and Annulments of Marriage. Ninth Annual Report. U. S. Department of Commerce, Bureau of the Census.*

U. S. Government Printing Office, Washington. 10 cents. $5\frac{7}{8}$ x $9\frac{1}{8}$; iii + 90; 1932 (paper).

The present report consists of an analysis of the records on marriage and divorce for the year 1930 and a comparison of these data with those of previous reports. There are 35 tables in the text giving the summarized data arranged in convenient form for comparisons and a brief digest of the causes for which divorces were granted in the various states during 1930. In the latter part of the report are given the detailed tables on divorces, classified by cause and by party and to whom granted, for geographical divisions and states for 1930, and also tables of marriages, divorces and annulments for 1930 and 1929, by counties.

The total number of marriages in 1930 was 1,128,280, a decrease of 104,279 or 8.5 per cent under the previous year. The data show that the marriage rate per 1000 of the total population has been declining from the highest point recorded (11.96 per 1000 in 1920), with one or two exceptions, to 1930 when the rate of 9.16 per 1000 occurred. This decline is attributed in part to a natural regression from the abnormally high rates in 1917, 1920 and 1923 together with the changes in the economic status of women. Economic factors undoubtedly are involved in the decline in 1930. The records show that in 1930 there were 191,591 divorces granted, a decrease of 4.9 per cent from the preceding year and fewer by 4,348 than occurred in 1928. Somewhat over two thirds of the divorces granted in 1930 were to wives. Comparing 1930 with previous years the proportion of divorces granted to the husband has been less than that shown for the earlier years. In general, the data exhibit a gradual decrease in the proportion of divorces granted for adultery and desertion with a corresponding increase in the proportion granted for cruelty. Unfortunately with

the wide differences existing between states in the relative importance of specified causes for divorce and the skillful handling of divorce cases by the legal profession the real cause for divorce is frequently concealed. Approximately two thirds of all divorces granted in 1930 were to couples that had been married less than 10 years. To such "couples were granted 65.2 per cent of the divorces for adultery, 69.7 per cent of those for cruelty, 58.9 per cent of those for desertion, 58.9 per cent of those for drunkenness, and 68 per cent of those granted for neglect to provide." Minor children were reported in 37.8 per cent of all divorce cases.

FIJI AND THE FIJIANS, 1835–1856.

By G. C. Henderson. Angus and Robertson, Sydney, Australia. $5.00. 6 x 9¼; xv + 333; 1931.

A modest contribution, limited in scope, but thoroughly scholarly within its set limits. With a colossal amount of original documentary research the author has reconstructed the history of missionary endeavor in the Fiji Islands from its beginning up to the late fifties of the last century, and the effects of this endeavor upon the Fijians. While Professor Henderson is perfectly fair to these early Methodists, Williams, Lyth, Hunt, and others, and indeed pays warm tribute to their heroism and devotion, he tells the sickening truth about what they did to a highly developed, flourishing, happy civilization living in a state of soundly established social equilibrium. The narrow bigotry, fanatical zeal, and vast ignorance of these misguided Wesleyans wrought devastating effects. Perhaps their souls rest in God; but if they do heavenly justice is even odder than the mundane variety.

This book is not for the general reader, but for the student. It is a contribution to the literature of general biology of first-rate importance.

HUXLEY.

By Clarence Ayres. W. W. Norton and Co., New York. $3.00. 5½ x 8⅛; vii + 254; 1932.

HUXLEY. *Prophet of Science.*

By Houston Peterson. Longmans, Green and Co., New York. $3.50. 5½ x 8½; xiii + 338; 1932.

Evidently the revival of interest in Darwin is being followed by a similar revival of interest in Huxley. In these two biographies Mr. Ayres gives the more straightforward narrative and the livelier impression of Huxley's personality, while Mr. Peterson is more concerned with an analysis of his work and of its relation to his predecessors, contemporaries and successors. Both books are furnished with bibliographies and indexes, while Mr. Peterson's contains as an appendix Huxley's last article, the hitherto unpublished second part of his reply to Arthur Balfour's *The Foundations of Belief*.

Of Huxley's three aspects, biologist, educator, and champion of freedom of thought, the last is perhaps of greatest interest today. His discoveries on the germ layers of the Medusae and on man's relation to the lower animals stand as foundation stones of embryology and anthropology; but, though foundations are important parts of a structure, one's interest turns to the newer levels of the edifice, where building is still in progress. Nor, except in Tennessee, does evolution now need a bulldog. The support of education by the state, the recognition of science in the curriculum, and the use of laboratory methods in the teaching of the sciences, which Huxley did so much to promote, are all established principles of the modern educational system. But the old war between authority and observation goes merrily on, though the terrain on which its battles are fought has changed.

A WANDERER IN INDO-CHINA. *The Chronicle of a Journey Through Annam, Tong-King,*

Laos, and Cambodgia, with Some Account of Their People.

By Hermann Norden. H. F. and G. Witherby, London. 15 shillings net. 5½ x 8½; 290; 1931.

That Indo-China is still a fertile field for the author-traveler this book bears witness. A peninsula something less than the size of Texas, has in it a population of upwards of twenty millions of people of whom fifteen millions are Annamites, twenty thousand are Europeans and the remainder represent mixtures of most of the blood of Asia. The great Annamite range with its glorious scenery and mountain peoples; the jungle with its ancient ruins and wild game; the great rice plantations; Saigon with its dash of modern France; and Cho'lon, a modern commercial Chinese city, furnished the author with a wealth of material. Particularly interesting to the reader will be the record of his wanderings in the mountains of the northern part of the peninsula, a region which is ordinarily little written about. Mr. Norden had the opportunity to observe much concerning tribal customs and the social life of the different tribes. It was in Laos that he witnessed the Feast of the Virgins, a carnival of love. Sexual expression before marriage is no disgrace to the Laos maiden but the punishment meted out to the unfaithful wife is heavy indeed.

The author has written a book of great interest to the general reader. Numerous illustrations from the author's own photographs and a map showing his wanderings add to the reader's enjoyment.

THE STORY OF MEDICINE.

By Victor Robinson. Albert and Charles Boni, New York. $5.00. 5¾ x 8¾; 527; 1931.

PATHFINDERS IN MEDICINE.

By Victor Robinson. Medical Life Press, New York. $10.00. 6¼ x 9¼; xvii + 810; 1929.

"Intellectually, we are but a stone's throw from the Stone Age; and emotionally we are still living there," Robinson says in the early pages of "The Story of Medicine," a fascinating delineation of the men who have dominated medical science during the course of its development. The truth of this statement is constantly emphasized as we see how full-abreast with great physicians of modern times, in essentials and outlook, stand the great physicians of the ancient world, such as Hippocrates, Aretaeus and Rhazes. How medicine survived the hocus-pocus and blind restrictions of the Middle Ages, to become, in the light of advances in physical and experimental knowledge, an increasingly more refined and dynamic science is narrated in graphic style. We only regret that the book could not have been illustrated.

This second and enlarged edition of "Pathfinders in Medicine" includes thirty essays, each dealing with an individual whose life and scientific contributions have stood out in the development of medical history. Numerous and excellent plates are included.

SORCERERS OF DOBU. *The Social Anthropology of the Dobu Islanders of the Western Pacific.*

By R. F. Fortune. With an Introduction by B. Malinowski. E. P. Dutton and Co., New York. $5.00. 5¾ x 8½; xxviii + 318; 1932.

After a five months stay at Tewara, a small island with forty inhabitants in the D'Entrecasteaux group, Dr. Fortune had so well acquired the language that nothing the natives said escaped him. He spent a month only at Dobu itself, which lies off the coast of Normanby. Whether or not an anthropologist can really gain a conception of a strange and hitherto little studied people in such a short time is open to question. Nevertheless it is to his credit that he got all of his information through direct observation and living with the natives.

Dr. Fortune's work is done in the latest anthropological style—the Functional method. He not only gives plenty of facts, but attempts to place details in their

proper order so that one can understand the tribal organization along with the emotional complexes, superstitions, fears and passions of the individuals making up the tribe. The art of living among the Dobuans consists in knowing a great deal of magic. Whether it is successful magic or not makes no real difference. The result is that these people are constantly suspicious of each other and there is little harmony even in the family. The culture in brief may be said to be puritanical, immoral, and incapable of change or progress, since any individual or group at all different from the rest is immediately done away with.

BRAVE NEW WORLD.

By Aldous Huxley. Doubleday, Doran and Co., Garden City, N. Y. $2.50. $5\frac{1}{4}$ x $7\frac{3}{4}$; 311; 1932.

This witty satire on certain present-day tendencies is laid in the year of Our Ford 632. Existence has been thoroughly stabilized, standardized, and trivialized. Parenthood and the family have long been obsolete; the citizens of this industrial utopia are produced by a process of ectogenesis and systematically conditioned for the functions of their respective castes. Sex has been put in its rightful place as one of the major pleasures. In accorddance with the words of Our Ford, "History is bunk," the study of obsolete cultures has been abolished. Art, religion and even science, which has made this world order possible, are fettered as potentially dangerous to stability. Into this standardized world is introduced a young man from one of the savage reservations, who has been reared on Shakespeare and Zuñi mythology. As might be anticipated, the results are disastrous for the Savage.

Unlike most creators of utopias, Mr. Huxley is evidently not in love with his creation. One wonders, however, whether his two alternatives are the only ones possible, whether there is no choice except between extreme standardization and extreme romanticism.

A YANKEE IN PATAGONIA, EDWARD CHACE.

By Robert and Katharine Barrett. Houghton Mifflin Co., New York. $4.00. $5\frac{3}{4}$ x $8\frac{1}{2}$; ix + 349; 1931.

On their travels in South America, the Barretts met Chace, a Yankee who had shipped to Patagonia thirty years before. A year later they met him again in Massachusetts where he had returned for a visit. By steps they drew his story from him and have transcribed it in this informal array of tales of strange adventures and experiences. Biologists will be interested especially in the first-hand observations on the breeding habits of the ostrich, *Rhea darwini*. There is much original and unique information on the behavior of ostriches, wild cattle, pumas, and guanacos, those early ancestors of the camel, both within their own groups and with respect to each other.

WOMAN IN PRIMITIVE MOTHERRIGHT SOCIETIES.

By J. H. Ronhaar. J. B. Wolters, Groningen, The Hague; David Nutt, London. 9 guilders (Holland); 15 shillings net (England). $5\frac{1}{2}$ x $8\frac{1}{2}$; iv + 541; 1931.

This book is written for the serious student only. The author emphasizes the importance of studying primitive peoples by the inductive method, a method which he finds too little used by ethnologists. That Dr. Ronhaar has pursued his researches on motherright societies with the utmost care and diligence the book amply testifies. The material is arranged somewhat in the form of a "catalogue, with added explanations." The views and theories of other workers are discussed extensively. After examining all the typical characteristics of matriarchy as reported in ethnological literature he reaches the conclusion that when tested with facts, these characteristics, whether many or few, cannot be accepted as indicative of a matriarchial system. The matriarchial structure erected by Bachofen and strengthened by McLennan and later writers he found to be based on an accumulation of fallacies. Great care

has been taken by the author in documentation. One is amazed, however, to find that no index was prepared. This omission seriously impairs the usefulness of the book.

SNOW MAN. *John Hornby in the Barren Lands.*

By Malcolm Waldron. Houghton Mifflin Co., New York. $3.00. $5\frac{1}{2}$ x 8; x + 292; 1931.

This account of the wanderings of two strange companions in the barren lands of Northern Canada is an amazing tale. The book has been compiled from the extensive notes and diaries which Captain J. C. Critchell-Bullock, a novice in Arctic travel, recorded. His companion, Jack Hornby, the son of a famous English cricket player, had been a wanderer from choice in the Arctic and sub-Arctic regions for more than twenty years. Together these two spent the winter months on the western edge of the barren lands and with the coming of spring journeyed eastward to Hudson Bay. The reader cannot but be both fascinated and horrified by the experiences and hardships which these two experienced. The character sketch of John Hornby pictures a man remarkable in many ways.

GROWTH IN PRIVATE SCHOOL CHILDREN, *with Averages and Variabilities Based on 3,110 Measurings on Boys, and 1,473 on Girls, from the Ages of One to Nineteen Years.*

By Horace Gray and J. G. Ayres. University of Chicago Press, Chicago. $3.50. 6 x 9; xv + 282; 1931.

Observations on the physical anthropology of the offspring of our socially and economically successful classes reveal that they are in some respects biologically significantly different from the offspring of the proletariat who have previously served as the guinea-pigs for most studies of this nature. The authors have treated their material with thoroughness and accuracy, and have taken pains to be explicit about the statistical methods and technique in taking measurements. The topics considered are, in brief, means of measurements for each age group; normal variations for each age; comparisons between private school children and other children; differences in racial stocks, economic levels, types of build, and abnormal degrees of fatness and thinness. Fifteen physical measurements on each of 3,110 boys and 1,473 girls serve as the basis for this constitutional study.

SELECTED MANUSCRIPTS OF GENERAL JOHN S. CLARK RELATING TO THE ABORIGINAL HISTORY OF THE SUSQUEHANNA.

Edited by Louise W. Murray. Society for Pennsylvania Archaeology, Frances Dorrance, Secy., 69 S. Franklin St., Wilkes-Barre, Pa. $2.00 (paper); $2.50 (cloth). $6\frac{3}{4}$ x 10; xvi + 150; 1931.

This is the first in a series of publications to be issued under the auspices of the Society for Pennsylvania Archaeology. General Clark's studies on the Iroquois led him to an intensive study of Indian sites in the Susquehanna valley, little of which was published during his lifetime, and which owes its present publication to the devotion of his pupil, Mrs. Murray. He identifies the Indian town, Carantouan, the stronghold of Champlain's allies against the Onondagas, with Spanish Hill, near Waverly, N. Y. The second part of the book is devoted to the Andastes or Susquehannocks. There are a bibliography and an index.

DIE MATERIELLE KULTUR DES KABULGEBIETES.

By Bruno Markowski. Verlag Asia Major, Leipzig. 17 marks (paper); 20 marks (cloth). 7 x 10; viii + 154 + 35 plates; 1932.

The material for this book was collected while the author was German teacher in the newly founded *Amanie Oberrealschule* in Kabul, Afghanistan from 1923–1926. Dr. Markowski presents to us a thorough and clear account of the geography of the Kabul valley, its climate, flora

and fauna, the history and development of the country, its various tribes, languages and religions, the structure of the buildings, clothing, food, domestic servants, cultivation of the soil, and its products, business, commerce, trades, standard of coinage, measures and weights. Incidentally we also get interesting glimpses into the social life and conditions of the people. Thirty-five valuable plates adorn the book.

PHYSIQUE AND INTELLECT.

By Donald G. Paterson. The Century Co., New York. $2.50. 5¼ x 7¾; xxvii + 304; 1930.

There has always been a general philosophical assumption that physical traits and mental traits must be in some manner associated. In a thorough and critical review of the literature, Paterson demonstrates the extremely shaky character of the evidence in favor of this assumption. In general, it appears that mental development is largely independent of physique; that it is unaffected by most physical handicaps or defects; that only diseases and defects directly involving the central nervous system seem to have any marked effects on mental development.

OSLER AND OTHER PAPERS.

By William S. Thayer. The Johns Hopkins Press, Baltimore. $3.50. 5¾ x 8¾; 386; 1931.

In this delightful volume Dr. Thayer has brought together biographical essays on Osler, Laennec, Richard Bright, and Cotton Mather in his medical aspects, together with addresses on medicine, medical education and other subjects. Although the contents are thus diverse, certain themes run through the various papers: Medicine as an art based on the physical and biological sciences; the importance of physical diagnosis, which laboratory methods should supplement, not supplant; the value of the classics in education; and, last but not least, *Aequanimitas*, that saving virtue which Osler both preached and practiced.

FIVE SCORE. *A Group of Famous Centenarians.*

By W. Forbes Gray. John Murray, London. 12 shillings net. 5½ x 8½; ix + 322; 1931.

Biographical sketches of twelve English centenarians, nine men and three women, chosen because they "achieved renown on other grounds than that of having defied the ordinary reckoning of human existence." On the whole, they are not a particularly inspiring lot; the best of them is Sir Moses Montefiore, the Jewish philanthropist; though there is something to be said for Bishop John Leslie, who married "when not far off his seventieth year. The lady . . . was only eighteen. . . . There were no fewer than ten children" the sixth son being born when Leslie was 79.

THE DECLINING BIRTH RATE IN ROTTERDAM. *A Statistical Analysis of the Drop in the Number of Children in 24,664 Rotterdam Families During the Last 50 Years.*

By J. Sanders. Martinus Nijhoff, The Hague. 6 guilders. 6½ x 10; x + 179; 1931.

Dr. Sanders' object in this book is to investigate the influence of occupation and religion on the decline in the birth rate and to determine how far the latter is a result of contraceptive practices. Unfortunately he presents his results in the form of number of children per family rather than of number of children per person-year exposure to risk of having a child. Many of his conclusions are therefore doubtful.

ARCHAEOLOGY OF KAUAI. *Bernice P. Bishop Museum Bulletin 80.*

By Wendell C. Bennett. Bernice P. Bishop Museum, Honolulu. $2.00. 6¾ x 10; iii + 156 + 15 plates; 1931 (paper).

It is unfortunate that the lack of good legendary material for Kauai, and the scarcity and inadequacy of the genealogical records make it impossible to tie up

the archaeological findings with historical facts. The few remains that are found on the 547 square miles of Kauai indicate that the old civilization must have differed in several respects from the other Hawaiian islands from which it is rather distantly separated. It is hoped that this study will prove stimulating to further investigations.

THE EVOLUTION OF THE HUMAN PELVIS IN RELATION TO THE MECHANICS OF THE ERECT POSTURE. *Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University, Vol. XI, No. 5.*

By Edward Reynolds. Peabody Museum, Harvard University, Cambridge. $1.00. $6\frac{1}{2}$ x $9\frac{1}{2}$; 80; 1931 (paper).

The author describes the various specializations of the pelvic girdle which have occurred among the mammalian orders as a result of different habits of life. There is a brief but exact explanation of the mechanical principles involved in locomotion. Special study is devoted to the marsupial kangaroos, edentate ant-eaters, small rodents, the bears, lower primates and anthropoid apes, since they all occasionally assume erect posture, and thus mark transitional stages between ordinary quadrupeds and man.

DIE NACHWIRKUNG DER KRIEGSHUNGERPERIODE AUF DAS SCHULKINDERWACHSTUM. *Eine schulärztlich-anthropometrische Wachstumsstudie nach Einschulungsuntersuchungen in Berlin nebst Anleitung zur statistischen Behandlung biologischer Reihenmerkmale.*

By Georg Wolff. Leopold Voss, Leipzig. 8.50 marks. $6\frac{5}{8}$ x $9\frac{5}{8}$; vii + 72; 1932 (paper).

An investigation on a total of 5,576 children entering the Berlin schools during the years 1924–1931. The results show that from 1924 to 1928 there was a statistically significant increase in both height and weight, which is attributed to the effects of undernourishment during the war. The years 1929–1931 show no significant changes from 1928, indicating that the economic difficulties of these years did not involve undernourishment of the children.

SINGLE FINGER PRINTS. *A New and Practical Method of Classifying and Filing Single Finger Prints and Fragmentary Impressions.*

By Harry Battley. Yale University Press, New Haven. $2.50. 6 x $9\frac{1}{2}$; 98; 1931.

It is a little annoying to find a book on finger prints that omits all mention of Francis Galton; particularly when Sir Edward Henry's name is carefully inserted into the opening sentence of the preface. Aside from this minor irritation, the book appears to present a workable solution of a problem that neither Galton nor Henry attempted seriously to deal with, namely, the classification of single finger prints.

HOLY PRAYERS IN A HORSE'S EAR.

By Kathleen Tamagawa. Ray Long and Richard R. Smith, New York. $3.00. $5\frac{7}{8}$ x 9; 264; 1932.

This autobiography of the daughter of a Japanese father and an Anglo-Irish-American mother is a highly readable narrative as well as a valuable document. The author does not approve of interracial marriages; yet to judge from her book both her parents' marriage and her own have been quite as successful as the average intraracial marriage.

THE ART OF EGYPT THROUGH THE AGES.

Edited by Sir E. Denison Ross. The Studio, London; William Edwin Rudge. New York. $15.00 (New York); 30 shillings (London). $8\frac{3}{4}$ x $11\frac{1}{4}$; 354; 1931.

A fascinating survey that is beautifully illustrated with about 300 plates. The text is well written by eminent authorities in Egyptian archaeology. The periods considered are the following: Predynastic and Early Dynastic; Old Kingdom; Inter-

mediate Period and Middle Kingdom; Hyksos Period and New Kingdom; Reign of Tutankhamen; Saïte, Ptolemaic and Roman Periods; Coptic Period; Muslim.

A Graphic Summary of American Agriculture Based Largely on the Census. *United States Department of Agriculture Miscellaneous Publication No. 105.*

Compiled by O. E. Baker. U. S. Government Printing Office, Washington. 60 cents. 5⅞ x 9⅛; 228; 1931 (paper).

A useful set of maps displaying much valuable information in regard to agriculture, based on the census of 1925. The largest section is devoted to land utilization and crops, but there is also much financial and other material.

Manual of the International List of Causes of Death. *Based on the Fourth Decennial Revision by the International Commission, Paris, October 16 to 19, 1929. U. S. Department of Commerce, Bureau of the Census.*

U. S. Government Printing Office, Washington. $1.25. 5¾ x 9; 342; 1931.

This fourth revision of the International List will be, of course, indispensable to vital statisticians. In addition to the list proper, there is a tabular list referring to their proper title the various causes that may be actually recorded on the death certificate. The index covers 165 pages.

A Biographical Index of Deceased British and Irish Botanists.

Compiled by James Britten and George S. Boulger. Second Edition, Revised and Completed by A. B. Rendle. Taylor and Francis, London. 15 shillings net. 5½ x 8½; xxii + 342; 1931.

A new edition of a well-known work, first issued in 1893. There are, we estimate, about 2,800 entries. It would have been interesting to have had included some small statistical analysis of the material. The book is a useful reference volume.

The Anthropometry of the American Negro.

By Melville J. Herskovits. Columbia University Press, New York. $4.00. 6¼ x 9½; xv + 283; 1930.

This is a presentation and detailed analysis of the material on which the author's *The American Negro, a Study in Racial Crossing*, reviewed in Volume III, page 436, was based. There is a bibliography of three pages but unfortunately no index.

Statistiques des Maladies à Déclaration Obligatoire pour l'Année 1929. *Publiées par la Section d'Hygiène. (Statistics of Notifiable Diseases for the Year 1929, Published by the Health Section.) League of Nations, Health Organization.*

World Peace Foundation, 40 Mt. Vernon St., Boston. 80 cents. 9⅝ x 12½; 81; 1931 (paper).

ZOOLOGY

The Grey Squirrel. *The Introduction and Spread of the American Grey Squirrel in the British Isles, its Habits, Food, and Relations with the Native Fauna of the Country.*

By A. D. Middleton. Sidgwick and Jackson, London. 4s. 6d. net. 4¾ x 7¼; viii + 107; 1931.

This book amply demonstrates that a serious mistake was made when the American grey squirrel was introduced into the British Isles. Mr. Middleton, who has made an exhaustive study of the habits of this rodent, does not reach any definite conclusion regarding the harm which the grey squirrel does to the native birds, or to the popular red squirrel. While they eat the eggs and nestling birds to some extent, yet, on the whole, the amount of harm really done is speculative. The decrease of the red squirrel he believes to be due largely to a cyclic fluctuation, which at present is little understood, but which has taken place regularly throughout the centuries, not only in populations of squirrels and other rodents but in many other animal groups as well. How

successful the red squirrel, now increasing in numbers and apparently definitely on the upward trend of its period cycle, will be in the presence of the grey squirrel seems uncertain.

Concerning the activities of the grey squirrel in relation to the interests of man the author writes with much concern. Farm products, forests, and game are all seriously disturbed and some method of control is imperative. Utilization of the grey squirrel for food and fur would largely help in solving the problem, but to the British public squirrel meat does not at present appeal, nor is there any attempt to utilize the skins.

The book will interest the general reader as well as the zoologist. Besides the economic problems, the author includes valuable material on the natural history of the grey and red squirrels.

THE MYSTERY OF THE MOA. *New Zealand's Avian Giant.*

By T. Lindsay Buick. (Published under the auspices of The Board of Maori Ethnological Research.) Thomas Avery and Sons, New Plymouth, N. Z. 15 shillings. 5 x 8; xvi + 357; 1931.

The first record of the moa, the extinct ostrich-like bird which attained the height of 12 feet, and formerly ranged the islands of New Zealand, appeared about 100 years ago. Since that time notes and articles have been published in various scientific and lay journals. Mr. Buick has meticuously searched the literature, especially old files of New Zealand newspapers and has retrieved much information concerning this mysterious bird that was in danger of being lost. It has been his aim to write not a scientific treatise but a history of the progress of the discoveries concerning the moa, and to discuss the theories which have been advanced for its presence on the islands, the reasons for its disappearance, and the time of its extinction. At least six genera and twenty-three species of the bird have been found. It ranged widely over the islands but its remains have been found in greatest numbers in the maritime and river plains. That some great catastrophe befell the moa in relatively recent times seems indicated by the large numbers of skeletons found massed together in various swamp lands of the islands. In one area fully 1000 birds representing fourteen species met death, probably at the same time. Mr. Buick suggests a frightful panic caused perhaps by fire.

The book is freely illustrated and contains a bibliography and an index. In an appendix are given notes referring to the text. The general reader will find this an absorbing book.

THE COCONUT MOTH IN FIJI. *A History of Its Control by Means of Parasites.*

By J. D. Tothill, T. H. C. Taylor and R. W. Paine. The Imperial Bureau of Entomology, London. 31 s. 6d. net. $7\frac{3}{4}$ x $10\frac{1}{2}$; vii + 269 + 33 plates; 1930.

One of the chief exports of the Fiji Islands is the oil obtained from copra, the dried meat of the mature coconut. For many years prior to 1925 this industry was seriously in danger of being ruined by the little purple moth, *Levuana iridescens* B.B., which attacks the leaves of the coconut. In 1924 the problem of finding a means of coping with this pest was assigned to a group of entomologists, who now publish this report. It deals with a study of the history of the control of *Levuana* up to 1925, a general account of the campaign instituted in January 1925, particularly the introduction of *Artona* parasites, and an extensive account of the life history, habits and relationships of *Levuana iridescens*. It contains also sections on the Tachinid fly, *Ptychomyia remota*; allied Zygaenids and their natural control; the Zygaenid parasite Chalcid B; the egg parasite *Trichogrammatoidea nana*, and the predacious beetle *Callimerus arcufer*.

It was the introduction of the parasites *Ptychomyia* and *Apanteles*, particularly the former, that crowned the committee's efforts with success. At the time the report was written (January 1929) there had been no new outbreak of *Levuana iridescens* for three years.

The volume is extensively illustrated

with figures and plates and contains an annotated list of the more important literature on *Levuana* published in Fiji, and an index.

CLASSIFICATION OF INSECTS. *A Key to the Known Families of Insects and Other Terrestrial Arthropods. Bulletin of the Museum of Comparative Zoology at Harvard College, Vol. LXXIII.*

By Charles T. Brues and A. L. Melander. Museum of Comparative Zoology, Harvard College, Cambridge. $5.50 (paper); $6.50 (cloth). 6 x 9; 672; 1932.

This book is a contribution of first-rate importance to the literature of systematic entomology. No student or teacher of the subject can afford to be without it. The book gives intelligently planned and workable keys to all the families of insects, and the families of some 18 classes of terrestrial arthropods not insects. It embodies the ripe wisdom and long experience of the authors in teaching and research. The present manual grew out of a "Key to the Families of North American Insects" privately printed some 17 years ago. In its present form the geographical limitation has been removed, brief but very well chosen lists of literature added, and many other features included to enhance its usefulness. There are 1,121 line illustrations of typical forms.

Altogether this is a fine and useful manual, which must have cost the authors a colossal amount of labor.

THE PRAIRIE HORNED LARK. *Transactions of the Academy of Science of St. Louis, Vol. XXVII.*

By Gayle B. Pickwell. Academy of Science of St. Louis, St. Louis. $2.00. 6 x 9; 160 + 34 plates; 1931 (paper).

After two years intensive study of the activities of the Prairie Horned Lark (*Otocoris alpestris praticola* Hensh.), the writer of this monograph finds that "the things yet to be learned seem more momentous by far than the few things learned." The origin of *praticola* he believes to be from a more southern lark since it exhibits in a more rigid climate the early breeding habits of these forms. Before the advent of the early settlers it ranged the great prairie regions. With the destruction of forests and the growth of farm lands its habitat was greatly increased. The larger part of this interesting report is devoted to the breeding habits and reproduction of the lark which the author has studied with great thoroughness. He includes numerous tables and figures exhibiting range of territory; range of temperatures and snow and breeding activities; locations and protection of nests; reactions to man when eggs or young were in the nest; daily activities of male and female during incubating period; loss of egg and nestlings during mating season; growth of nestlings, etc. The work includes a bibliography and a group of 34 plates showing habitats and nests.

LIFE-HISTORIES OF THE FROGS OF THE OKEFINOKEE SWAMPS, GEORGIA. *North American Salientia (Anura) No. 2.*

By Albert H. Wright. The Macmillan Co., New York. $8.00. $6\frac{3}{4}$ x $10\frac{3}{8}$; xv + 497; 1931.

The results of a thorough study of the life-histories of Anura indigenous to the south-eastern United States are outlined in this book. Following a review of the previous pertinent amphibian explorations (to 1922) and a description of the Okefinokee region, the Okefinokee Salientia in general are discussed with regard to morphology, breeding habits, development, natural history, etc. The remainder, and bulk, of the book takes up in detail the life-histories of the different species: their common names, range, general appearance, coloration, structural characters, measurements, habitat, general habits, voice, mating, ovulation, eggs, hatching period, the mature tadpole, the larval period, transformation, growth, food, enemies, autumnal disappearance and affinities. The information is colossal in scope and is presented in concise, well-organized form. The forty-five plates are excellent. An outstanding contribution to American zoölogy.

CHECK-LIST OF BIRDS OF THE WORLD. *Volume I.*

By James L. Peters. Harvard University Press, Cambridge. $3.50. 5¾ x 9; xviii + 345; 1931.

This check-list is along the lines of Sharpe's *Handlist*, except that it is somewhat more elaborate and far more extensive. When completed it will undoubtedly become the standard work of its kind and will long serve as a manual in all taxonomic work in ornithology. Volume I includes the ostriches, rheas, casuaries, emus, apteryxes, tinamous, penguins, loons, grebes, albatrosses, diving petrels, shearwaters, petrels, herons, flamingos, swans, geese, ducks, American vultures, hawks, and falcons. The author follows the International Code of Zoölogical Nomenclature. The normal range of each bird is completely outlined. Within each genus the species have been arranged in phylogenetic sequence. Subspecies are arranged geographically from north to south and from west to east. References for each genus and species have been selected with meticulous care. The arrangements of the table of contents and the detailed index enhance the usefulness of the book.

FLORIDA BIRD LIFE. *Published by Florida Department of Game and Fresh Water Fish, in coöperation with Bureau of Biological Survey, United States Department of Agriculture.*

By Arthur H. Howell. Color plates from original paintings by Francis L. Jaques. Coward-McCann, New York. $6.00. 7¼ x 9¾; xxiv + 579 + 58 plates; 1932.

Florida has long been the happy hunting ground for bird lovers. Stretching as it does in a long narrow peninsula towards the tropics, having an irregular coast line with many tidal lagoons, and dotted with lakes and marshes, it is the home of a great variety of land and water birds and the favorite migratory lane for many species that journey further south to the West Indies and South America. The author of this book, Senior Biologist of the United States Bureau of Biological Survey, has produced a thoroughly comprehensive and authoritative treatise on the bird life of Florida. Those interested in avifauna will find it delightful reading and an invaluable guide. The handsome format in which the volume is issued, together with the well chosen illustrations, particularly the colored plates from the paintings of F. L. Jaques, form an appropriate setting for the subject matter of the text.

ZOÖLOGY. *A Textbook for College and University Students.*

By F. E. Chidester. D. Van Nostrand Co., New York. $3.75. 5¾ x 9; xii + 581; 1932.

This text-book of modern zoölogy embodies a discussion of the various animal forms from the protozoa through the mammals. In the preface the author states: "Some of the most important features of the book are the logical arrangement of facts about the animals within a group, a statement of the chief characteristics at the beginning of the discussion, and a summary on the economic importance at the end of each section. The newer physiology has been introduced and a bibliography checked by experts in each field is given at the end of each chapter."

The book was written as a general survey for the college student and as a reference book for the biologist. It seems needlessly detailed for the former purpose, and somewhat too general adequately to fulfill the latter.

MEDICAL ENTOMOLOGY. *A Survey of Insects and Allied Forms Which Affect the Health of Man and Animals.*

By William A. Riley and Oskar A. Johannsen. McGraw-Hill Book Co., New York. $4.50. 5¾ x 9; xi + 476; 1932.

A thorough treatise on insect forms which have influenced the health of man in the past and present. Each chapter contains

a key and good illustrations for purposes of identification; also considerable information about protection and extermination of the more dangerous insects. One chapter is devoted to an account of the structure and development of insects. We predict that this will become a standard text, and have a long career of success and usefulness.

THE SOCIAL LIFE OF MONKEYS AND APES.

By S. Zuckerman. Harcourt, Brace and Co., New York. $3.75. 5½ x 8½; xii + 356; 1932.

This book is of much broader scope than its title indicates. For in addition to its penetrating description and analysis of anthropoid behavior it is a mine of information on the comparative physiology of sex and of reproduction. It is written with thorough first-hand knowledge and insight, both of the animals discussed and the literature. The bibliography includes over 400 titles. The book is a sound, substantial contribution of permanent value, which it is impossible to recommend too highly.

FUNDAMENTALS OF INSECT LIFE.

By C. L. Metcalf and W. P. Flint. McGraw-Hill Book Co., New York. $4.00. 5¾ x 9; xi + 581; 1932.

This volume based on the authors' larger work, *Destructive and Useful Insects*, has been prepared as a textbook for classes in entomology. Emphasis is placed on the biological rather than the agricultural aspect of the subject. The first ten chapters of the original work have been revised and brought up to date and much new material included. Three new chapters have been added dealing with the biology and ecology of insects and insect behavior. The many well chosen illustrations in the text and the bibliography of 97 titles add much to the usefulness of the book. There is a detailed index.

THE BIBLIOGRAPHY OF SCHISTOSOMIASIS (*Bilharziasis*), *Zoological, Clinical and Prophylactic. The Egyptian University, The Faculty of Medicine, Publication No. 1.*

By Mohamed Bey Khalil. Egyptian University, Cairo. 8¾ x 9½; x + 506; 1931 (paper).

Although this bibliography contains 2,649 references, arranged both alphabetically by author and under a detailed classification of subjects, Dr. Khalil does not regard it as complete and urges all interested workers to communicate any additional references or proposed alterations to be incorporated in a second edition. Even as it stands, however, helminthologists and students of tropical medicine owe him a large debt of gratitude.

ANIMAL BIOLOGY.

By Michael F. Guyer. Harper and Bros., New York. $3.75. 5½ x 8⅜; xii + 744; 1931.

An introduction to biology which attempts to establish in the mind of the student a philosophical attitude towards biology. "Emphasis is laid upon those biological facts which are of greatest importance in the development of human thought and culture." An excellent textbook for the beginner.

PARAMECIUM DAS PANTOFFELTIERCHEN. *Eine Monographische Zusammenfassung der wichtigsten Kenntnisse.*

By Hans Kalmus. Gustav Fischer, Jena. 10 marks (paper); 11.50 marks (cloth). 8½ x 9⅝; iv + 188 + 3 plates; 1931.

A useful monograph, covering taxonomy, morphology, physiology, ecology, demography, laboratory husbandry, and experimental technique. It should be of interest to biologists generally, and invaluable to anyone working on the infusoria. The usefulness of the bibliography (833 titles) is increased by brief notes supplementing many of the titles. The index is excellent.

TRAITÉ DE ZOOLOGIE. *Fascicule IX. Les Oiseaux.*

By Edmond Perrier and Rémy Perrier. Masson et Cie, Paris. 40 francs. 6½ x 10; 224; 1931 (paper).

Volume 8—on reptiles—of this series was noticed in these columns in Volume 4. The tenth, and last, volume—on mammals—is now in press. The present addition to this comprehensive reference work on zoölogy gives a good account of the anatomy, morphology, skeletal structure, physiology and classification of birds.

UNINVITED GUESTS. *A Short Account of the Animals Living On or In Us.*

By David Causey. Alfred A. Knopf, New York. $2.00. 5 x 7¼; ix + 120; 1932.

A popular account of the more important parasites of man. Their life histories and their manner of getting in and out of their hosts are presented in an interesting as well as authoritative manner. The illustrations are worthy of note. While accurate, they sometimes take on an almost human appearance.

INSECT PESTS OF FARM, GARDEN AND ORCHARD.

By E. Dwight Sanderson. Third Edition, Revised and Enlarged by Leonard M. Peairs. John Wiley and Sons, New York. $4.50 net. 5¾ x 9; vii + 568; 1931.

The third edition of a book which first appeared in 1912 has been thoroughly revised and enlarged by the junior author who is professor of entomology at West Virginia University. Much new material has been included, particularly concerning the methods of coping with insect pests, and the number of illustrations has been greatly increased.

VIVIPARUS. *Morphometrie, Phylogenie und Geographie der europäischen, fossilen und rezenten Paludinen.*

By V. Franz. Gustav Fischer, Jena. 24 marks. 11 x 14; 160; 1932 (paper).

A study of the European fossil and recent Paludinae. The biometric technique is open to some criticism. Until the data are reworked by sounder statistical methods it will be doubtful whether different races really have the differences in form here attributed to them.

STATISTICAL REVIEW OF THE ALASKA SALMON FISHERIES. *Part III: Prince William Sound, Copper River, and Bering River. U. S. Department of Commerce, Bureau of Fisheries, Bulletin No. 7.*

By Willis H. Rich and Edward M. Bull. U. S. Government Printing Office, Washington. 20 cents. 7⅜ x 11; 60; 1932 (paper).

The authors find wide variation in the catch of different years, the fish being predominantly four and five years old at maturity. This may be compared with Hjort's finding of wide variation in the age cohorts of North Sea cod and herring. In spite of intensive fishing there is little evidence of depletion.

SEA FISHES AND SEA FISHING IN LOUISIANA *including Recipes for the Preparation of Sea Foods. State of Louisiana, Department of Conservation, Bulletin No. 21.*

By James N. Gowanloch and J. B. Dauenhauer, Jr. Department of Conservation, New Orleans. Free. 6 x 9; 187; 1932 (paper).

An entertaining popular account of Louisiana fishes, their natural history, and their gastronomic possibilities. The recipes are eminently sound; it is interesting to note that wine makes its appearance in the proper places.

THE LIFE OF THE BUTTERFLY.

By Friedrich Schnack. Translated by Winifred Katzin. Houghton Mifflin Co., Boston and New York. $2.50. 5 x 7½; 278; 1932.

This is a translation from the German original, noticed in Volume V, page 248.

We can commend it to all who enjoy Maeterlinck and other literary naturalists.

A Bibliography of the Sardines. *Division of Fish and Game of California, Bureau of Commercial Fisheries. Fish Bulletin No. 36. Contribution No. 111 from the California State Fisheries Laboratory.*

By Genevieve C. Wheeler. California State Fisheries Laboratory, Terminal Island, Calif. Free. 6 x 9; 135; 1931 (paper).

Ciliates from Bos Indicus Linn. *II. A Revision of Diplodinium Schuberg. University of California Publications in Zoölogy, Vol. 37, No. 5.*

By C. A. Kofoid and R. F. MacLennan. University of California Press, Berkeley. $1.00. 7 x 10¾; 100; 1932 (paper).

Gigantomonas Lighti Sp. Nov., A Trichomonad Flagellate from Kalotermes (Paraneotermes) Simplicicornis Banks. *University of California Publications in Zoölogy, Volume 37, No. 6.*

By Frank H. Connell. University of California Press, Berkeley. 40 cents. 7⅛ x 10¼; 36 + 2 plates; 1932 (paper).

Four New Species of Haematoloechus (Trematoda) from Rana Aurora Draytoni from California and Cephalogonimus Brevicirrus, A New Species of Trematode from the Intestine of Rana Aurora from California. *University of California Publications in Zoölogy, Vol. 37, No. 7, and No. 8.*

By Lloyd G. Ingles. University of California Press, Berkeley. 30 cents. 7 x 10¾; 28; 1932 (paper).

The Life-cycle of Babesia Bigemina (Smith and Kilbourne) of Texas Cattle-fever in the Tick Margaropus Annulatus (Say). *With Notes on the Embryology of Margaropus. University of California Publications in Zoölogy, Volume 36, No. 11.*

By Emery W. Dennis. University of California Press, Berkeley. 60 cents. 7⅛ x 10¾; 36 + 6 plates; 1932 (paper).

Type Localities of Birds Described from California. *University of California Publications in Zoölogy, Volume 38, No. 3.*

By Joseph Grinnell. University of California Press, Berkeley. $1.00. 7⅛ x 10¾; 82 + 1 map; 1932 (paper).

Where the Nineteenth Century Whaler Made His Catch. *Bulletin New York Zoölogical Society, Vol. XXXIV, No. 6.*

By Charles H. Townsend. New York Zoölogical Society, New York. 30¢ for single copy of Bulletin; $1.50 a year. 7½ x 10¼; 6 + 1 folding map; 1931 (paper).

BOTANY

Sulphur Bacteria. *A Monograph.*

By David Ellis. Longmans, Green and Co., New York. $7.50. 5½ x 8¾; ix + 261; 1932.

The researches on sulphur bacteria are spread over a wide field of investigators and are described in various languages. An attempt is made in this book to present to English readers a summing up of the present knowledge of the subject. Natural sources of sulphuretted hydrogen and its assimilation by bacteria are discussed, and the metabolism of the sulphur bacteria and methods of culture are described. A consideration of various classifications is followed by descriptions of the characteristics of the bacteria coming under the two main groups, *Leucothiobacteria* (colorless sulphur bacteria) and *Rhodothiobacteria* (colored sulphur bacteria). Chapters are devoted to the intimate structure of the cell, to irritability, and to the mechanics of ciliary movement. In the final chapter, material on the color-

ing matter of the sulphur bacteria is reviewed. The illustrations are excellent, and a full bibliography is included.

PRINCIPLES OF SOIL MICROBIOLOGY. *Second Edition, Thoroughly Revised.*

By Selman A. Waksman. The Williams & Wilkins Co., Baltimore. $10.00. 6 x 9; xxviii + 894; 1932.

The second edition of this highly useful book has undergone considerable modification in order to bring it up to date. The chapters dealing with the mycorrhiza fungi and the soil as a medium for plant and animal parasites have been entirely rewritten. New chapters have been added dealing with the rôle of microörganisms in the decomposition of organic matter in green manures and stable manures, in the formation and decomposition of peat and forest soils, and with the relation between plant growth and the activities of microörganisms in soil. The original size of the volume has been maintained by condensation of some of the chapters and elimination of unnecessary material.

LEHRBUCH DER PFLANZENPHYSIOLOGIE. *Zweiter Band. Stoffaufnahme. Stoffwanderung. Wachstum und Bewegungen.*

By S. Kostytschew and F. A. F. C. Went. Julius Springer, Berlin. 28 marks (paper); 29.80 marks (cloth). 6¼ x 9½; vi + 459; 1931.

The first volume of this work was reviewed in these columns in Vol. I, and its English translation in Vol. VI. The present volume of this standard treatise deals with the foundations of the physiology of the food metabolism of plants; water economy; movement of sap; translocation and distribution of nutritive materials; growth; and tropisms. It is hoped that an English translation of this second volume will be published soon.

EXPERIMENTAL PLANT PHYSIOLOGY.

By George J. Peirce. Henry Holt and Co., New York. $1.50. 5½ x 8½; ix + 166; 1931.

This is not a laboratory manual but a description of experiments that furnish the evidence on which conceptions of plant physiology are based. If the reader has facilities for carrying out the experiments, so much the better; if not, the mere reading of the book will give him some idea of the factual basis of the subject.

SOIL CONDITIONS AND PLANT GROWTH. *Sixth Edition.*

By E. John Russell. Longmans, Green and Co., New York. $7.50. 5½ x 8½; viii + 636; 1932.

The fifth edition of this book was reviewed here in Volume II, Number 4. The present edition contains 120 more pages than the last and the revision has been much more extensive than a mere addition of sections covering the later investigations. In the new edition it remains an excellent summary of our present knowledge of an extremely complex field.

MAN AND MICROBES.

By Stanhope Bayne-Jones. The Williams & Wilkins Co., Baltimore. $1.00. 5 x 7¼; x + 128; 1932.

In this number of the *Century of Progress Series* Professor Bayne-Jones traces in broad outline our present knowledge of the bacteria, protozoa, fungi and viruses. The word "microbe" is apt to bring to one's mind the pathogenic micro-organisms. The book, however, treats the useful and essential activities of the microbes, their part in the cycle of the elements, their use in baking, brewing, and other industries, as well as their devastating effects.

THE CACTUS AND ITS HOME.

By Forrest Shreve. The Williams & Wilkins Co., Baltimore. $3.00. 5½ x 8½; ix + 195; 1931.

An account of the life habits, methods of cultivation, and uses of the cactus, with some natural history of our desert regions thrown in. It is hoped that cactus fan-

ciers will be stimulated by this book to investigate further the curious phenomena exhibited by these extraordinary plants.

ELEMENTARY BACTERIOLOGY.

By Joseph E. Greaves and Ethelyn O. Greaves. W. B. Saunders Co., Philadelphia. $3.50 net. 5¼ x 7½; 535; 1932.

As an elementary text in bacteriology, this book is both stimulating and comprehensive. An historical account of the development of the science is given; the nature and classification of the bacteria are considered; and the pathogenic and non-pathogenic rôles of the organisms in the life of man are described. The book is well illustrated.

INVESTIGATIONS ON THE SPIKE-DISEASE OF SANDAL. *III. Report of Progress Made During the Quarter Ending 20th September 1931. IV. Report of Progress Made During the Quarter Ending 31st December 1931.*

Contributors: C. Dover, H. A. H. G. Hicks, J. E. M. Mitchell, M. Sreenivasaya, Y. V. Sreenivasa Rao, A. V. Varadaraja Iyengar. Edited by V. Subrahmanyan, for the Sandal-Spike Working Committee. Indian Institute of Science, Hebbal, Bangalore. 7¼ x 9¾; 20; 1931.

ACTA FORESTALIA FENNICA 37. *Publications of the Society of Forestry in Suomi, Helsinki.* 6⅜ x 9½; 496; 1931 (paper).

SIIKAKANKAAN MÄNTYTAIMISTOJEN TUHOISTA (*Über die Schädigungen der Kiefernpflanzenbestände in Siikakangas*). *Silva Fennica 17.*

By Esko Kangas. Society of Forestry in Suomi, Helsinki. 6½ x 9¾; 107 + 6 pp. cuts + map; 1931 (paper).

SUUNNITELMA SUOMEN LAUTTAUSV¨YLILLÄ UITETUN PUUN UPPOAMISEN TUTKIMISESTA. (*Vorschlag zu einer Untersuchung des Senkholzverlustes in den Triftstrassen Finnlands*). *Silva Fennica 19.*

By I. Lassila. Society of Forestry in Suomi, Helsinki. 6½ x 9¾; 48; 1931 (paper).

DIE ORGANISATION DER FORSTWISSENSCHAFTLICHEN FORSCHUNGSARBEIT IN FINNLAND. *Vortrag auf dem 3. Nordischen Forstkongress in Oslo am 1. August 1930. Silva Fennica 20.*

By A. K. Cajander. Society of Forestry in Suomi, Helsinki. 6½ x 9¾; 11; 1931 (paper).

TERVANPOLTON LEVIÄMINEN SUOMESSA 1700-LUVUN PUOLIMAISSA. (*Tar-Burning in Finland in the Middle of the 18th Century*). *Silva Fennica 21.*

By E. E. Kaila. Society of Forestry in Suomi, Helsinki. 6½ x 9¾; 38; 1931 (paper)

LES SPADICIFLORES DU QUÉBEC. (*Aracées, Lemnacées*). *Contributions du Laboratoire de Botanique de l'Université de Montréal—No. 19.*

By Frére Marie-Victorin. Université de Montréal, Montreal. 75 cents. 6 x 9; 60; 1931 (paper).

MORPHOLOGY

DER SCHWERPUNKT DES LEBENDEN MENSCHEN. *Schwerpunktwage und Schwerpunktpendel.*

By Adolf Basler. Gustav Fock, Leipzig, and Verlag der Sun-Yatsen-Universitaet, Canton. 9 marks. 7 x 10½; 100; 1931 (paper).

Two apparatuses for studying the position of the center of mass of living bodies are described in detail. 1. The intersection of a line dropped perpendicularly through the center of gravity with a horizontal plane surface supporting the body is determined by means of a modification of

du Bois-Reymond's weighing apparatus. The position of this intersection is found for a variety of standing, crouching, and kneeling positions. 2. A second apparatus, in principle based upon the declination of a gravity pendulum when a body of known weight is superimposed upon it, is described for determining the absolute distance of the center of gravity of the body from a fixed point of support. The absolute and relative positions of the center are determined for various positions of a single individual.

A STUDY OF HAIRS AND WOOLS *Belonging to the Mammalian Group of Animals, Including a Special Study of Human Hair Considered from the Medico-Legal Aspect. Egyptian University, Faculty of Medicine, Publication No. 2.*

By John Glaister. Misr Press, Cairo. $7\frac{3}{4}$ x 11; 188 + 145 plates; 1931.

The chief merit in this book lies in the fact that three-fourths of it is devoted to excellent microphotographic plates. There are 110 plates of mammalian hairs, ranging from those of the *Platypus* to those of the gorillas, and 35 additional plates of human hairs. Considerable space is devoted to the legal advantages that one would derive from an ability to identify stray hairs picked up around the scene of any crime; but, on the whole, the sections concerned with description and classification are more satisfactory.

GRUNDZÜGE EINER KONSTITUTIONSANATOMIE.

By Walter Brandt. Julius Springer, Berlin. 28 marks (paper); 29.80 marks (cloth). $6\frac{3}{4}$ x $10\frac{1}{8}$; ii + 382; 1931.

A noteworthy and thoroughly sound addition to the literature of the constitutional anatomy of plants and animals. It treats of the time and space factors in comparative embryology; conditioning of structural patterns; environmental and constitutional factors influencing growth and polymerisation; and differentiation. The author plans to follow this book with a *Allgemeine Konstitutionsanatomie des Menschen* and a *Spezielle Konstitutionsanatomie* dealing with organs and systems of the body.

VERTEBRATE EMBRYOLOGY. *Revised.*

By Robert S. McEwen. Henry Holt and Co., New York. $4.75. $5\frac{1}{2}$ x $8\frac{1}{2}$; xi + 581; 1931.

This is a revised edition of the author's introductory text on vertebrate embryology. The subject is presented in five parts. Part I presents modern theories of cytology, fertilization, and early stages in development. In Part II the development of the frog is discussed. Part III deals with segmentation and gastrulation in teleosts and gymnophiona. Part IV is devoted to the development of the chick, and Part V to mammalian embryology.

LEITFADEN FÜR HISTOLOGISCHE UNTERSUCHUNGEN AN WIRBELLOSEN UND WIRBELTIEREN.

By Hans Hoffmann. Gustav Fischer, Jena. 12 marks (paper); 13.50 marks (cloth). $6\frac{3}{4}$ x $10\frac{1}{8}$; viii + 200; 1931.

An introduction to comparative histology, designed for use in the "grosse zoologische Praktikum," as a kind of supplement to Kükenthal's textbook. Histological technique is not covered. There is a short general chapter, followed by sections dealing with the various phyla. The forms chosen are mainly those treated in Kükenthal.

PHYSIOLOGY AND PATHOLOGY

TRAITÉ DE PHYSIOLOGIE NORMALE ET PATHOLOGIQUE. *Tome II. Alimentation et Digestion.*

By E. Bardier, G. Battez, H. Bierry, Léon Binet, P. Carnot, P. Combemale, C. Delezenne, A. Desgrez, R. Gayet, R. Glénard, L. Hallion, M. Lisbonne, A. Pi Suner, M. Vagliano, E. Wollman. Masson et Cie, Paris. 100 francs. $6\frac{1}{2}$ x $9\frac{1}{2}$; 565; 1931.

This volume in the great French physiological text currently appearing exhibits clearly the difficulties and the advantages of such composite works, where different specialists contribute the several chapters. The simple matter of documentation illustrates the point. Some chapters carry adequate bibliographical references; others are practically devoid of them.

The arrangement of the material in this volume is as follows: Foods and rations; vitamines; hunger; thirst; salivary glands; stomach; intestine; the external secretion of the pancreas; alimentary absorption; microbes and microbial activity in the alimentary tract; movements of the alimentary tract—prehension, mastication and deglutition; movements of the stomach and intestines.

A worthy and valuable contribution to the scholarly apparatus of French medical teaching.

The Development of the Mammary Gland as Indicated by the Initiation and Increase in the Yield of Secretion. *College of Agriculture, Agricultural Experiment Station Research Bulletin 156.*

By Charles W. Turner. University of Missouri, Columbia. 6 x 9; 49; 1931 (paper).

A Study of the Causes of the Normal Development of the Mammary Glands of the Albino Rat. *College of Agriculture, Agricultural Experiment Station Research Bulletin 157.*

By C. W. Turner and A. B. Schultze. University of Missouri, Columbia. 6 x 9; 45; 1931 (paper).

The Relation of the Anterior Pituitary Hormones to the Development and Secretion of the Mammary Gland. *College of Agriculture, Agricultural Experiment Station Research Bulletin No. 158.*

By C. W. Turner and W. U. Gardner. University of Missouri, Columbia. 6 x 9; 57; 1931 (paper).

A Determination of the Blood and Plasma Volume of Dairy Cattle. *A Study of Blood and Plasma Volume During Growth, Pregnancy, and Lactation. College of Agriculture, Agricultural Experiment Station Research Bulletin No. 159.*

By C. W. Turner and H. A. Herman. University of Missouri, Columbia. 6 x 9; 61; 1931 (paper).

The Anatomy of the Mammary Gland of Cattle. *Fetal Development. College of Agriculture, Agricultural Experiment Station Research Bulletin No. 160.*

By Charles W. Turner. University of Missouri, Columbia. 6 x 9; 39; 1931 (paper).

Reports of a series of studies initiated for the purpose of investigating the mechanism and factors which influence milk secretion in cattle. In view of the fact that the dairy cow plays an important rôle in industry, as well as in maintaining the health of the nation, it is surprising that so little has been known of the anatomy of the udder, or of the physiology of milk secretion, and the various hormones involved.

Manuel de Coprologie Clinique. *Deuxième Édition.*

By R. Goiffon. Preface by J.-Ch. Roux. Masson et Cie, Paris. 20 francs. 5 x $7\frac{5}{8}$; xi + 260; 1925 (paper)

Rabelais, and Montaigne after him, would have loved the idea of a serious, precise, documented, illustrated and completely scientific "Manual of Coprology,"—an excrementitious encyclopedia in short. Well, here it is; and in a second edition, so useful has the wisdom it disseminates proved to be! For more than ten years the author has devoted himself assiduously to the researches embodied in this volume. As the Master of a local Grange once said, in introducing a lecturer from the agricultural college who was to speak on fertilizers, "he is plainly full of his subject." And, as Doctor Roux so truly points out in his preface, systematic and thorough coprologic examination and analysis "est toujours, en effet, un travail difficile."

For the clinician this is a really valuable guide and reference work. The matter is sound, up-to-date, and presented with the clarity and logical precision of the French teacher of science at his best.

Finally, the volume is so beautifully printed and illustrated that it would adorn the shelves of any gentleman interested in collecting the farther-flung oddities of literature.

THE WISDOM OF THE BODY.
By Walter B. Cannon. W. W. Norton and Co., New York. $3.50. 5½ x 8½; 312; 1932.

The physiologist often appears to be a man whose sole interest is in the heat produced by an isometric contraction of the gastrocnemius muscle of a frog, or in some similar problem which to a non-physiologist seems equally remote from anything of real interest. Somehow the process of analysis often seems to result merely in cluttering up the landscape with small and disconnected results, whose importance is not clear except to their authors. In the present book Dr. Cannon has performed a work of synthesis of a high order of merit. His general theme is the stability of the body and the mechanisms by which it is maintained. For the biologist whose primary concern is with some other field than mammalian physiology, and for the intelligent layman, this book will prove both pleasant and profitable reading.

BLOOD SUGAR IN NORMAL AND SICK CHILDREN *with Special Reference to Coeliac Disease.*
By Elisabeth Svensgaard. Levin and Munksgaard, Copenhagen. Dan. Cr. 10. 6½ x 10; 245; 1931 (paper).

Data on blood sugar in adults are fairly complete, but there have been few systematic observations on young children. This reprint covers the behavior of blood sugar in the new born, infants, and older children (1–13 years) both normal and sick. Infants (15 days–1 year) show the greatest variation in blood sugar after ingestion of glucose. Coeliac diseases, cretinism, eczema, rickets, and tetany were studied with a view to finding out if children with these diseases presented a picture different from normal children. The group of coeliac diseases showed marked difference in blood sugar regulation. These children had a low blood sugar curve after ingestion of glucose. The other groups showed no difference.

HEALTH. *A Practical Book on Hygiene. By George S. Foster and Leon G. Kranz. Christopher Publishing House, Boston.* $3.00. 5½ x 8½; 218; 1932.

This book contains sound, practical, advice for the laymen, couched in simple language. The latest views on the proper treatment of the muscles, intestines, feet, teeth, hair, and other organs are briefly outlined. The benefits of exercises are highly extolled, though no magic formula is given to make bending to the floor twenty times an interesting occupation. There is the usual caution about the use of those pleasant substances tobacco and alcohol.

PHYSIOLOGY OF EXERCISE.
By Ferdinand A. Schmidt and Wolfgang Kohlrausch. Authorized Translation from the German by Carl B. Sputh, Assisted by Emil Rath and Leopold Zwarg. F. A. Davis Co., Philadelphia. $2.50. 5¾ x 8⅝; 216; 1931.

Various systems of the body—skeletal, muscular, nervous, respiratory and circulatory—are considered from the point of view of their physiology and their potential reaction to different types and amounts of exercise. Exercises best suited for different age groups are discussed. An interesting chapter is devoted to constitutional and athletic types.

LES VITAMINES.
By Lucie Randoin and Henri Simmonet. Armand Colin, Paris. 10.50 francs (paper); 12 francs (cloth). 4½ x 6¾; 220; 1932.

An account of the discovery and nature of the vitamines. Experimental work, chemical, physical and biological, is cited. The practical aspects of the present ex-

tending knowledge of the vitamines in the field of nutrition are discussed.

La Maladie de Hodgkin. *Lymphadénie Aleucémique. Lymphosarcome. Pseudo-Leucémie de Bonfils. Morbus Hodgkini de Wilks. Adénie de Trousseau. Granulome Malin de Benda. Lymphogranulome Malin. Lymphogranulomatose Maligne, etc.*

By Paul Chevallier and Jean Bernard. Masson et Cie, Paris. 75 francs. 6¾ x 10¼; 293; 1932.

A comprehensive review of Hodgkin's Disease in its pathologic and clinical aspects. Respecting the many vexing questions that exist concerning this syndrome, the author ventures no decided personal views, but a fair hearing is given to all the current views on the subject. The book is clearly and interestingly written. There is a selected bibliography and an index.

Les Maladies Organiques du Faisceau de His-Tawara. *Les Syndromes Coronaires. L'Endocardite Septale. L'Infarctus Septal. (Étude Clinique et Anatomique).*

By Ivan Mahaim. Preface by L. Michaud. Masson et Cie, Paris. 80 francs. 6½ x 10; vi + 595; 1931 (paper).

The first and second parts of this volume are taken up with a systematic account of the anatomy, physiology and pathologic affections of the His-bundle. The third part considers the relation of these to other cardiac pathology. There is a generous bibliography and an index.

Précis de Bactériologie Médicale.

By André Philibert. Masson et Cie, Paris. 50 francs (paper); 65 francs (cloth). 5¼ x 7½; xi + 551; 1931.

A useful handbook for the bacteriologist and medical student. It includes directions for bacteriological technique and descriptions of the microbes pathogenic to man. It is well illustrated and has an index.

Origin of Cancer. *The Specific Cancer Cell of Carcinoma, Contrasted with the Normal Matrix Embryonal Cellule of Primal Ovum Days, to 8th Week; from which the Cancer Cell is Directly Descended. Cancer Not Epithelial. Sarcoma Not Connective Tissue.*

By Frank A. Stahl. Frank A. Stahl, Hamilton Club, Chicago. 6 x 9⅛; 96; 1932 (paper).

BIOCHEMISTRY

Ergebnisse der Enzymforschung. *I. Band.*

Edited by F. F. Nord and R. Weidenhagen. Akademische Verlagsgesellschaft, Leipzig. 27 marks (paper); 29 marks (cloth). 6 x 9¼; xi + 377; 1932.

This new serial, having for its purpose the critical exposition of the current development of research on enzymes, starts off auspiciously. Its quality and usefulness is sufficiently indicated by the following list of subjects and authors included in this first volume: Thermodynamics of cell reactions, by Fulmer; Significance of oxidation-reduction potentials for enzyme reactions, by Wurmser; Mechanism of enzyme action, by Fodor; Physico-chemical events in enzyme reactions, by Nord; Alcoholic fermentation, by Harden; Proteolytic enzymes, by Grassmann; Specificity of carbohydrases, by Weidenhagen; Bacterial enzyme reactions, by Quastel; Acetic fermentation, by Bertho; Tyrosinase, by Raper; Bone phosphatase, by Robinson; Crystalline urease, by Sumner; Crystalline pepsin, by Northrop; the oxygen transferring enzyme of respiration, by Reid; Biochemistry of the lower fungi, by Raistrick; The coming-into-action of plant enzymes, by Suessenguth.

We bespeak the support of biologists generally for this new enterprise.

Colloid Chemistry. *Theoretical and Applied. Vol. III. First Series of Papers on Technological Applications.*

By Selected International Contributors. Collected and Edited by Jerome Alexander.

The Chemical Catalog Co., New York. $10.50. 6 x 9; 655; 1931.

Volumes I and II of this work were reviewed in Volume IV of this journal. This volume deals chiefly with the technological applications of colloid chemistry. The first eleven papers are concerned with general principles and are followed by six papers outlining the mechanical phases. These papers will interest many large industries. The second group of twenty five papers the editor calls "Telluric" as the authors discuss colloid chemistry in relation to geology, mineralogy, metals, petroleum, asphalt, and agriculture.

An able international group of scientists have contributed to this volume and have succeeded in reaching the high standards set by the two previous volumes.

FORTSCHRITTE DER BIOCHEMIE. *II. Teil* (1924–1931).

By Felix Haurowitz. Theodor Steinkopff, Dresden and Leipzig. 11 marks (paper); 12 marks (cloth). 6 x 8¾; x + 152; 1932.

A review of the author's conspectus of the most important work done in biochemistry from 1914 to 1924 appeared in Volume II of this journal. The present volume covers the work done in this field between 1924 and 1931. During this period much important and fundamental work has been published in animal biochemistry, especially on glycolysis and respiration, muscle contraction, sex-hormones, and vitamins A and D. More than 800 bibliographical titles are noted. A solidly useful book.

COLLOIDS.

By Ernest S. Hedges. Longmans, Green and Co., New York. $4.50. 5½ x 8½; vii + 272; 1931.

Although this textbook is intended more especially for students of chemistry, the author has endeavored to treat the subject in a sufficiently broad way to interest students of physics, biology and geology. Numerous references, both to classical papers and to recent developments of special interest, are incorporated in the text.

PHYSICAL CHEMISTRY. *An Elementary Text, Primarily for Biological and Pre-medical Students.*

By Louis J. Gillespie. McGraw-Hill Book Co., New York. $2.75. 5½ x 8; ix + 287; 1931.

An elementary textbook, which deals primarily with chemical equilibrium. In order to make for unity, the subject matter has been limited to a discussion of the conditions of equilibrium, and the velocities involved in reaching an equilibrium. Biological students would do well to study this book and get an idea of some of the physical phenomena inevitably operating in biological processes.

THE GLYCOSIDES.

By E. F. Armstrong and K. F. Armstrong. Longmans, Green and Co., New York. $4.25. 6 x 9½; vii + 123; 1931.

Detailed and clear explanations of the nature, structure, and biological significance of sugars are neatly stated, and accompanied with numerous configuration formulae. A valuable reference source for both the biochemist and the plant physiologist.

SEX

LA THÉORIE PHYSICO-CHIMIQUE DE LA SEXUALITÉ.

By Ph. Joyet-Lavergne. Gaston Doin et Cie, Paris. 10 francs. 4½ x 6⅞; 103; 1932 (paper).

The author of this extremely interesting little book is professor at the Lycée Condorcet, Maître de Conférences à l'École normale supérieure de Sèvres, and Directeur-adjoint à l'École Normale des Hautes Études. He knows the literature of the biology of sex thoroughly, and has done considerable first-hand

research in the field. His physico-chemical theory of sex is not in opposition to either the chromosomal or the hormonal theories. It seeks to present a more general conception of the problem. This conception regards the sex chromosomes and the hormones as secondary manifestations of sexuality, expressions of certain physiological states which are fundamentally determined by the physico-chemical qualities of the cells.

The author's own researches have led to the formulation of two "laws" of cytoplasmic sexualization, as follows:

1. The value of the intra-cellular oxidation-reduction potential (*r*H) is a characteristic of the sexualization of the cytoplasm; within a species, the cells polarized in the female direction have an *r*H lower than that of the cells polarized in the male direction.

2. The differences in the nature and proportions of the lipoid and fat reserves constitute a characteristic of the sexualization of the cytoplasm; cells polarized in the female direction acquire fat reserves which reduce osmic acid; the lipoid reserves of the cells which will produce male gametes lack this quality.

These statements are believed to be of wide generality. In his discussion of the literature the author takes account of a wide range of established observations on sexuality, many of which are now generally neglected or even wholly forgotten by the zealous devotees of the philosophy of the gene. It will do no biologist any harm to read this little book.

CONTROL OF CONCEPTION. *An Illustrated Medical Manual.*

By Robert L. Dickinson and Louise S. Bryant. The Williams & Wilkins Co., Baltimore. $4.50. 6 x 9; xii + 290; 1931.

THE PRACTICE OF CONTRACEPTION. *An International Symposium and Survey.*

Edited by Margaret Sanger and Hannah M. Stone, with a Foreword by Robert L. Dickinson. The Williams & Wilkins Co., Baltimore. $4.00. $5\frac{3}{4}$ x $8\frac{7}{8}$; xviii + 316; 1931.

These two volumes bring to the practicing and teaching physician all the most recent information concerning contraception that is now available. The first volume, an illustrated medical manual, is issued to members of the medical profession only. It is an excellent compilation of present day knowledge concerning means and medical indications for the control of conception. The drawings have been executed with Dr. Dickinson's well known skill.

The second volume contains papers and discussions given at the Seventh International Birth Control Conference held in Zurich, September 1930. Dr. Dickinson contributes a preface and Margaret Sanger an introduction.

DRESS, DRINKS, AND DRUMS. *Further Studies of Savages and Sex.*

By Ernest Crawley. Edited by Theodore Besterman. Methuen and Co., London. 12 s. 6 d. net. $5\frac{1}{2}$ x $8\frac{3}{4}$; x + 274; 1931.

These three essays contain a great deal of entertaining as well as accurately detailed information about social behavior. The treatment of the origins and adaptations of dress is extremely sound, and the author's remarks on drink indicate, we think, an objective and unbiased approach to his subject.

> Many peoples, low in the scale of culture, emphasize by law the natural aversions of childhood, not to speak of womanhood, to intoxicants. ... And we may suitably conclude with an allusion to the one really notable instance of such a prohibition in modern civilization, that of the United States of America.

DONNÉES ACTUELLES SUR L'HORMONE TESTICULAIRE (*Modes d'obtention et de caractérisation*).

By L. Cuny and D. Quivy. Masson et Cie, Paris. 16 francs. $5\frac{1}{2}$ x 9; 76; 1932 (paper).

The existence of a testicular hormone was one of the first things about internal secretions to be known, but less has been learned about it than about the secretions of several other endocrine glands. In

the seven brief chapters of this book, the nature of the testicular hormone and the methods by which the active principle is prepared are discussed, and the physical and chemical properties of the hormones considered.

FEMALE SEX HORMONOLOGY. *A Review. By William P. Graves. W. B. Saunders Co., Philadelphia.* $3.50. 5¾ x 8⅞; 131; 1931.

This book incorporates a valuable, concise exposition of the present status of research on the hormones controlling menstruation, parturition and lactation in the human female. Brief descriptions of the experimental work on which modern theories are based are given. A full glossary and a competent bibliography are appended.

BIOMETRY

IL METODO STATISTICO. *Teoria e Applicazioni alle Scienze naturali, alle Scienze sociali, all'Arte. By Alfredo Niceforo. Casa Editrice Giuseppe Principato, Messina.* 75 lire. 6¾ x 9¾; 813; 1931 (paper).

Treatises on statistics, excellent from the standpoint of theory, are not difficult to find either in English or in any other language. And yet, with but few exceptions, in them we find a lack of cohesion between the various degrees of analysis, and they usually do not give a comprehensive view of the method as a whole. This pitfall Professor Niceforo has avoided. His exposition is at once clear and complete. Having defined statistics, its means and ultimate finality, he illustrates each progressive step in the theory with innumerable applications to problems taken from the most diverse fields. This is the characteristic and distinctive merit of this work, due to the author's rare ability to perceive and consider problems in every scientific field and to grasp the possibilities of statistics to deal with these problems.

Besides applications to demography and economics, some of which are original themes developed in other writings, he applies the method to various questions in botany, zoology, psychology, anthropology, art, etc. These are not presented purely as exercises but are given to prove the universality of statistics as the necessary means to understand the different manifestations of phenomena. And this the author achieves by presenting each problem in all its theoretical aspects, placing the question to be resolved, and showing how with statistical analysis this solution can be reached. Thus the specialist in each field is made aware of the benefits of statistics. The author has given special consideration to an important problem not commonly noted in other books, i.e., the study of simultaneous and multiple characters of one individual or group of individuals as compared with those of the whole population. For example: Given the height, weight, chest circumference, etc., of the individuals of a certain population, and having calculated the statistical constants for each character, we wish to compare one individual or group of individuals with the whole population as regards all the characters. Professor Niceforo proposes a simple graphic method to establish this comparison, a method which he promises to develop further in other works.

Although in various parts of this treatise it is apparent that the author realizes the necessity of a radical innovation in the exposition of the theory in regard to statistics of non-quantitatively measured data, yet he dedicates only one chapter to it. This we feel is not sufficient and a still greater contribution would have been made had he treated this part as fully as the rest.

MANUEL DES CALCULS DE LABORATOIRE. *Précision. Discussion et Interprétation des Résultats Expérimentaux à l'Usage des Chimistes, Biologistes et Physico-Chimistes. By H. Vigneron. Preface by P. Pascal. Masson et Cie, Paris.* 40 francs. 6⅛ x 9⅛; 183 (paper).

This excellent manual deals with the

theory of errors, the calibration of laboratory apparatus, graphics, curve fitting, interpolation, the simplification of arithmetic and algebraic operations, and the application of theoretical formulae to experimental results. It is clearly and sensibly written and gives a large amount of valuable information in a comparatively small space.

THE QUEEN OF THE SCIENCES.

By E. T. Bell. The Williams & Wilkins Co., Baltimore. $1.00. 5 x 7¼; 138; 1931.

In this number of the *Century of Progress Series* Professor Bell aims to explain to the general reader the broad outlines of the mathematical advances of the past hundred years. The reader will, we think, agree that his success in this difficult task is greater than might have been anticipated.

HANDBOOK OF STATISTICAL NOMOGRAPHS, TABLES, AND FORMULAS.

By Jack W. Dunlap and Albert K. Kurtz. World Book Co., Yonkers-on-Hudson, N. Y. $6.00. 7½ x 11; vii + 163; 1932.

The contents of this book are well arranged and the formulas are written in a uniform notation. It will be useful to the statistician, especially to the educational statistician.

PSYCHOLOGY AND BEHAVIOR

THE MIND AND ITS BODY. *The Foundations of Psychology.*

By Charles Fox. Harcourt, Brace and Co., New York. $3.50. 5½ x 8½; xii + 316; 1931.

The problem of the relationship between mind and body is approached in this book in the light of recent physiological and psychological research. The experiments of Cannon, Pavlov, and Lashley, especially, are cited as a challenge to three of the chief prevailing dogmas which have arisen under the influence of the theories of Descartes; namely, (1) that the reflex is the sole type of nervous activity, (2) that there is a point-to-point correspondence between mental events and brain events, and (3) that the cerebral cortex is the organ of conscious life. Experimentation not only has not established these dogmas, the author states, but has even shown them to be untenable, and observation is far outrun by theory. The doctrines of temperament, mental energy, instinct, and mental imagery are discussed in regard to their bearing on the mind-body problem. The author feels that the phenomenon of subjective activity presupposes a spiritual world from which the motive force is derived. The book concludes as follows:

> . . . . when a man contemplates the world of values the permanent effects on him are purely subjective. His being is steeped and dyed in what he experiences; so that nobility of soul arises from the pursuit of what is of permanent worth. And it is the destiny of man to foster such commerce with ultimate values in the confident hope that what is so acquired, as it does not depend on the body, can never pass away.

Perhaps a second volume, which the author promises, approaching "the most vital aspect of the mind-body problem, the immortality of the soul," will be more convincing on this score than the present work.

KÖRPERLICH-SEELISCHE ZUSAMMENHÄNGE IN GESUNDHEIT UND KRANKHEIT. *Ein Beitrag zur medizinischen Psychologie.*

By Walter H. v. Wyss. Georg Thieme, Leipzig. 6.50 marks. 5¾ x 8¾; x + 128; 1931 (paper).

A contribution to the study of the influence of psychological factors in man on the physiological and *vice versa*, in health and disease. The author treats especially of the questions concerning the psycho-somatic connections on a biological basis. He makes use not only of the results of physiological research but also of the ideas of a philosophically oriented psychology. The book is concerned first with the psychic functions in general and the human organism; then, in particular, with the emotions, their

expression and the interactions between the two, and between the psyche and the body, particularly the heart activity; and ends with a discussion of the experience of sickness and the influence of age, sex, environment, bodily influences, and the inner personal history of the patient upon it.

This book was written for physicians, but psychologists and biologists also should find it of interest. It has a competent index.

A FURTHER STUDY OF THE DELAYED REACTION IN THE ALBINO RAT. *Comparative Psychology Monographs, Vol. 8, No. 2, Serial No. 37.*

By Walter G. McAllister. The Johns Hopkins Press, Baltimore. $1.50. $6\frac{7}{8}$ x 10; 103; 1932 (paper).

An investigation of the behavior phenomena many animals present which enable them to return to a particular spot where something necessary to their existence has been previously concealed. Two questions are discussed: (1) the length of time over which an animal can delay, and then make the correct response, (2) the mechanism which enables an animal to make this response correctly. The maximal delay period obtained was $11\frac{1}{2}$ seconds, the median time of delay on the correct runs where the animal made 80 per cent correct responses over 40 consecutive trials.

There is some indication that the animal was led to make the correct response by a type of intra-organic cue. It is also highly probable that bodily orientation is not a significant factor in this phenomenon of animal behavior.

CHILDREN'S BEHAVIOR PROBLEMS. *A Statistical Study Based upon 5000 Children Examined Consecutively at the Illinois Institute for Juvenile Research. I. Incidence, Genetic and Intellectual Factors.*

By Luton Ackerson. University of Chicago Press, Chicago. $4.00. 6 x 9; xxi + 268; 1931.

This volume deals with the influence of age and intelligence on frequency of personality and behavior problems in children. Data from case records of 5000 children examined by the Illinois Institute for Juvenile Research are subjected to careful and conservative statistical analysis. The results of this analysis suggest the hypothesis that

> the interaction of the age factor and the intelligence factor produces the resultant effect of increasing the number of behavior problems within the lower age and IQ ranges, and of inhibiting them within the upper age and IQ ranges within our group of children below 18 years of age. It is probable also that either the age factor or the intelligence factor alone, if the other factor were held constant, would likewise produce this "waxing and waning" effect.

YALE LABORATORIES OF COMPARATIVE PSYCHOBIOLOGY. *Comparative Psychology Monographs, Vol. 8, No. 3, Serial No. 38.*

By Robert M. Yerkes. The Johns Hopkins Press, Baltimore. 75 cents. $8\frac{3}{4}$ x 10; 33; 1932 (paper).

In this report Dr. Yerkes outlines the development of the Yale Laboratories of Comparative Psychobiology and the problems which are being studied by the staff of investigators. The laboratories are constituted by "three geographically separate centers of activity: (1) New laboratory and primate quarters in conjunction with the laboratories of neurophysiology of the Yale School of Medicine, New Haven; (2) an anthropoid breeding and observation station at Orange Park, Florida; and (3) arrangements for cooperative use of temporary and permanent scientific stations in Africa where the chimpanzee, gorilla and other primates may be studied in their native habitat." At present all research activities are directed towards acquiring information concerning the chimpanzee. Many different lines of work are under way which in time will undoubtedly yield results of the greatest importance.

RELIGIOUS BEHAVIOR. *An Introduction to the Psychological Study of Religion.*

By David M. Trout. The Macmillan Co., New York. $4.00. 5¾ x 8½; xiv + 528; 1931.

As the title implies, this is a study of religious behavior rather than religious experience. However, the author does not take the extreme behavioristic position; he regards experience as "a subjective index of intraorganismic processes which is approximately as useful to the scientific student as the symptomatic introspections of the patient are to the physician." To Dr. Trout "any mode of behavior which is conceived as contributing in considerable degree to the achievement of the most valued goals one knows is for that individual a religious act." The rooter for dear old Siwash or the ardent member of the American Association for the Advancement of Atheism is not ordinarily regarded as religious, but there is no amendment to the Constitution forbidding Dr. Trout to make this grouping if it seems to him the most appropriate. The book includes a bibliography of 310 titles and name and subject indexes.

REWARD AND PUNISHMENT IN ANIMAL LEARNING. *Comparative Psychology Monographs, Vol. 8, No. 4, Serial No. 39.*

By Edward L. Thorndike. The Johns Hopkins Press, Baltimore. $1.25. 6¾ x 10; 65; 1932 (paper).

In these experiments Dr. Thorndike used the method of a "multiple-choice apparatus in which certain responses (a) are rewarded by freedom and food, certain others by (b) confinement for 30 seconds, and certain others by (c) what may be called thwarting. The subjects of the experiments were chicks 13 to 50 days old." Dr. Thorndike finds that the "results of all comparisons by all methods tell the same story. Rewarding a connection always strengthened it substantially; punishing it weakened it a little or not at all." These experiments also show the fallacy of the doctrine that ordinarily animal learning is due to the greater frequency of one connection.

AN EXPERIMENTAL STUDY OF PREHENSION IN INFANTS BY MEANS OF SYSTEMATIC CINEMA RECORDS. *Genetic Psychology Monographs, Volume X, Nos. 2–3.*

By H. M. Halverson. Clark University Press, Worcester, Mass. $3.00. 6½ x 9¾; 179; 1931 (paper).

A close analysis of the prehensile reactions of infants of 16, 20, 24, 28, 32, 36, 40 and 52 weeks of age to a cube placed before them according to a standard technique. The results distinguishing the different age-groups are discussed and interpreted in the light of the anatomical growth of the digits, the maturation of the neuromusculature, the part played by training, and the possibilities of increased cutaneous sensibility of the finger-tips. The work is a further careful contribution along the line which has characterized the previous investigations reported from the Yale Clinic of Child Guidance.

THE HISTORY OF LANGUAGE.

By Henry Sweet. J. M. Dent and Sons, London and Toronto. 1s. 9d. net. 3¾ x 6; xi + 148.

This is a reprint of an excellent introduction to comparative philology. The author concludes that the Aryan languages have affinities with the Ugro-Altaic family and with Sumerian. There is a bibliography but no index.

A HISTORY OF PSYCHOLOGY IN AUTOBIOGRAPHY. *Volume II.*

By Benjamin Bourdon, James Drever, Knight Dunlap, Giulio C. Ferrari, Shepherd I. Franz, Karl Groos, Gerardus Heymans, Harold Höffding, Charles H. Judd, C. Lloyd Morgan, Walter B. Pillsbury, Lewis M. Terman, Margaret F. Washburn, Robert S. Woodworth, Robert M. Yerkes. Edited by Carl Murchison. Clark University Press, Worcester, Mass. $5.00. 6 x 9; xvii + 407; 1932.

The first volume of this series was reviewed in Volume VI, page 249.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

INTRODUCTION TO THE HISTORY OF SCIENCE. *Volume II. From Rabbi Ben Ezra to Roger Bacon. (In two parts.) Carnegie Institution of Washington Publication No. 376.* *By George Sarton. The Williams & Wilkins Co., Baltimore.* $12.00. 7 x 10; xxxv + xvi + 1251; 1931.

This second volume of Dr. Sarton's colossal undertaking covers the twelfth and thirteenth centuries. Its sheer bulk is a little appalling; there are 111 pages of index alone. The purpose of the book is thus described in the preface:

> Critics who like to dispose of another man's work with a single label, should not call mine a dictionary or a bibliography but rather, if they please, a map—a scientific map with full indication of the sources. Such a comparison will help to appreciate its limitations. When the cartographer engaged in the compilation of a map discovers that the height of a mountain or the course of a river is uncertain, he cannot drop everything and start on a journey of exploration, or else his task would never be accomplished. The best that he can do is to state frankly the uncertainties he has come across; I have done that, but in many cases I have not been able to resist the temptation of indulging in little explorations of my own, and this may explain, if it does not justify, my increasing slowness.

The period covered in this volume is an interesting one, representing as it does the period during which Western Christendom was absorbing the achievements of Muslim civilization. Our knowledge of Muslim science is still far less complete than we should like; but as Sarton shows both in his first volume and in this one far more of our civilization than is commonly realized traces back to a Muslim origin.

REPORT OF THE LIBRARIAN OF CONGRESS *for the Fiscal Year Ending June 30, 19* *Library of Congress. U. S. Government Printing Office, Washington.* $1.00. x 9; vii + 463; 1931.

The chief item of interest to scientists in this report is the summary of the scientific books contained in the Vollbehr collection of incunabula.